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"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit.* lib. i. cap. 1. Not.

"Meditationis est perscrutari occulta; contemplationis est admirari perspicua . . . . . Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

———————

—" Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

                    J. B. Pinelli ad Mazonium.

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PLATES.

I. *On the Construction of certain new forms of Thermo-Barometers. By* T. Tate, *Esq.**

THESE instruments are highly useful on account of being more sensitive than the mercurial barometers. The thermo-barometer now commonly used is called a Sympiesometer by the instrument makers; it consists of an upright tube having a globe at the top bent downwards, and a cup at the lower extremity bent upwards; the globe is filled with hydrogen gas; and a liquid, usually strong sulphuric acid, stands in the tube and cup. The variations of the pressure of the atmosphere are indicated by the elevation of the liquid in the tube, a correction being made for the change of temperature. For this purpose there are two scales, one moveable, called the barometrical scale, the other fixed, called the thermometrical scale; the latter is graduated into degrees of temperature, and the former into equal divisions representing inches and tenths of a mercurial column. Now the construction of this instrument is not correct in principle; for it is constructed on the assumption that the variations of atmospheric pressure are in proportion to the changes in the height of the column of liquid. The globe is bent downwards to facilitate the graduation of the thermometrical scale, which is used for giving the correction for temperature: the globe being plunged into a water-bath, brought to different degrees of heat, corresponding marks are then made on the scale on a level with the liquid in the tube. Now it will be observed that, in this process, the gas occupying the tube is not brought to the temperature of the water-bath; whereas to have a correct scale of temperature, the whole volume of gas, as well as the column of liquid, should be equally heated;

* Communicated by the Author.

but to effect this with a sufficient degree of precision, in such a position of the globe, would not only involve considerable practical difficulties, but would also require the aid of a cathetometer. This constitutes a serious defect in the instrument, for the points in this scale should be determined with the greatest possible precision.

In order to avoid these difficulties and sources of error, I have constructed a simple instrument of this kind in the following manner :—

The instrument consists of a glass tube, A B, inserted in a light half-pint flask A, the connexion at *e v* being made air-tight ; a thermometrical scale *m n* attached to the tube ; a moveable barometrical scale C D ; Q a piece of very thin india-rubber tied over the top of the tube A B, to keep the external air from coming in contact with the strong sulphuric acid occupying a portion of the tube and flask. The tube A B is about 24 inches long, and about $\frac{3}{8}$ths of an inch internal diameter ; it is secured to the flask in the following manner :—Two perforated corks, *e* and *v*, fitting the neck of the flask, are placed on the tube ; the lower one, *e*, is coated with a solution of india-rubber to render it impervious to air ; the upper cork, *v*, being raised a little, the cork *e* with the tube is pressed down to its proper depth ; some pieces of chemical cement, fusible at a gentle heat, are now placed above the cork *e*, and a gentle heat is applied until the cement melts ; the upper cork *v* is then brought down to its place upon the melted cement ; when the cement has cooled, the tube will be found *firmly* fixed in the flask and perfectly air-tight. Strong sulphuric acid of commerce is introduced through the tube by means of a pipette, the tube being held in an inclined position.

N.B. All this process of cementing may be obviated by having a globe and tube bent in the manner shown in the succeeding diagram ; but I preferred showing how the instrument may be constructed cheaply, and with materials which may be readily obtained.

To form the scale *m n*, a narrow strip of paper, about 10 inches long, may be attached to the middle portion of the tube by means of a cement of liquid glue. The scale D C may be formed of a thin lath covered with paper, about 10 inches in length ; it may be simply applied by the hand, or it may be made to slide against an upright board placed at the back of the tube. These scales are graduated in the following manner :—

The scale *m n* must be graduated at a time when the weather

is settled, and when the mercury in the barometer indicates mean pressure, or nearly mean pressure, that is, about (p), or 29·5 inches. The liquid must be made to stand about the middle of the stem at mean temperature (t), or 62°. The flask is then placed in a water-bath, which is brought to different degrees of temperature (t_1), say to 42°, 62°, and 82°, and marks are made on the paper $m\,n$ coincident with the level of the liquid in the stem at these different temperatures; the spaces between these marks are respectively divided into twenty equal parts, thereby forming intervals of 1 degree. These divisions may be extended, if desirable, as the intervals are very nearly uniform (see formula 6).

The lower part of the barometrical scale D C must be graduated at a time when the weather is settled, and when the mercury in the barometer stands at a high column (p_2), the temperature of the air being (t) at, or not far from, the mean temperature. Having placed a mark (p) about the middle of the scale D C, bring this mark coincident with the temperature t on the scale $m\,n$, and make a mark on D C coincident with the level of the liquid in the tube; then the space, $-q_2$, between these two marks will indicate a change of atmospheric pressure measured by the column of mercury equal to $p-p_2$. Substitute the values $-q_2$ and $p-p_2$, thus obtained, in equation (4), and determine the value of the constant $\dfrac{\mathrm{V}}{a}$, the values of s and h having been previously found by experiment and observation. Substitute the value of $\dfrac{\mathrm{V}}{a}$, thus determined, in equation (5), and calculate the values of q_2 for $p_2 = 30$, 30·5, and 31 ; these values of q_2 being marked off from the middle point p on the scale D C, will give the points corresponding to these pressures; these spaces may then be subdivided into equal parts so as to read off tenths and hundredths. In precisely the same manner the upper part of the scale must be graduated, when the mercury in the barometer stands at a low column.

The observations of atmospheric pressure are made as follows :—The temperature of a delicate thermometer being first noted, the middle point p of the barometrical scale D C is moved until it coincides with this temperature as marked on the thermometrical scale $m\,n$; then the mark on the scale D C coincident with the level of the liquid in the tube, will give the atmospheric pressure as measured by a column of mercury.

Constructed in this manner, the errors of the indications cannot exceed ·03 of an inch of a column of mercury. Thus, by any person possessing ordinary skill in manipulation, an instrument may be made for a few shillings, which will be quite as accurate

in its indications as an ordinary barometer costing about thirty shillings, and certainly much more sensitive.

Let p = the pressure of the atmosphere when the gradations for temperature are made; h = the height of the column of liquid in the tube above the level of liquid in the flask, corresponding to t temperature, V volume and P pressure of the gas in the flask, p being constant; q_1 = the change of the height of the column at t_1 temperature, V_1 volume and P_1 pressure, p being constant; q_2 = the change of the height of the column (estimated from the last) at t_1 temperature, V_2 volume and P_2 pressure, and p_2 atmospheric pressure; a = the section of the tube; b = the section of the liquid in the flask; s = the specific gravity of mercury, that of the liquid being unity;—then assuming the section of the tube to be uniform, we find

$$P_2V_2 = P_1V_1;$$

but

$$V_1 = V + aq_1; \quad V_2 = V + a(q_1+q_2); \quad P = p + \frac{h}{s};$$

$$P_1 = p + \frac{1}{s}\left(h + q_1\frac{b-a}{b}\right); \text{ and } P_2 = p_2 + \frac{1}{s}\left(h + q_1\frac{b-a}{b}\right) + \frac{1}{s}q_2\frac{b-a}{b};$$

hence we get

$$p - p_2 = q_2 \times \frac{1}{s}\left\{\frac{sp + h + q_1\dfrac{b-a}{b}}{\dfrac{V}{a} + q_1 + q_2} + \frac{b-a}{b}\right\}. \quad . \quad . \quad (1)$$

When b is very large as compared with a, we may take $\dfrac{b-a}{b} = 1$, and then

$$p - p_2 = q_2 \times \frac{1}{s}\left\{\frac{sp + h + q_1}{\dfrac{V}{a} + q_1 + q_2} + 1\right\}, \quad . \quad . \quad . \quad (2)$$

where the signs of q_1 and q_2 are + when measured upwards, and − when measured downwards.

This formula expresses the change of atmospheric pressure in terms of the variables q_1 and q_2. It is obvious that $p - p_2$ is not *exactly* in the ratio of q_2, although it is so very nearly; for the value of the quantity within the brackets is but slightly affected by any *possible* values which may be given to the variables q_1 and q_2.

Neglecting q_1 and q_2 within the brackets, we obtain the approximate formula

$$q_2 = \frac{s(p - p_2)\dfrac{V}{a}}{sp + h + \dfrac{V}{a}}. \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Supposing $p-p_2$ and q_2 to be determined by observation, q_1 being neglected, or $t_1=t$, the constant $\dfrac{V}{a}$ may be found from (2), viz.

$$\frac{V}{a}=q_2 \times \frac{sp_2+h+q_2}{s(p-p_2)-q_2}. \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Again, solving equation (2) for the value of q_2, we get

$$q_2 = \tfrac{1}{2}\left\{\sqrt{4s(p-p_2)\frac{V}{a}+\left(sp_2+h+\frac{V}{a}\right)^2}-\left(sp_2+h+\frac{V}{a}\right)\right\}, \quad (5)$$

which is the formula I have employed for graduating the barometrical scale by giving different values to p_2.

By a similar mode of investigation we find, neglecting the pressure of the vapour of the liquid,

$$t_1-t=q_1\times\frac{t+458}{sp+h}\left\{(sp+h+q_1)\frac{a}{V}+1\right\}. \quad . \quad (6)$$

Here it will be observed that q_1 is very nearly in the ratio of t_1-t, that is to say, the graduations on the thermometrical scale mn are very nearly uniform. Neglecting q_1 within the brackets, and solving the equality for q_1, we find

$$q_1=\frac{(t_1-t)(sp+h)}{(t+458)\left\{(sp+h)\dfrac{a}{V}+1\right\}}. \quad . \quad . \quad (7)$$

This formula enables us to determine approximately the range of the thermometrical scale, having given the capacity of the flask, &c.; thus let

$$\frac{a}{V}=\frac{1}{180}, \; s=7{\cdot}5, \; p=29{\cdot}5, \; h=12, \; t=62^\circ, \; t_1=92^\circ,$$

then we find $q_1=5{\cdot}8$; again, for $t_1=32^\circ$, the other quantities being as before, we find $q_1=-5{\cdot}8$; therefore the range $=11{\cdot}6$ inches. In like manner formula (3) enables us to determine approximately the range of the barometrical scale, having given the range of the mercurial barometer; thus let $p_2=31$ for the lower part of the scale, and $p_2=28$ for the upper part, the other quantities being as before, then we find the entire range $=10$ inches nearly. Hence the length of the tube should not be less than $21{\cdot}6$ inches. The range of this instrument, indicating atmospheric change of pressure, is about three times that of the common barometer.

The instrument which I shall now describe has a range of $7\frac{1}{2}$ times that of the common barometer, and is at the same time strictly mathematical as regards the principle of construction,

and therefore free from those errors which necessarily arise from an empirical principle of construction, such as that adopted in the construction of the foregoing instrument. I have used this new instrument for some time, and find that its indications closely agree with those of the common barometer, excepting when the atmosphere is in an unsettled state, and then the want of agreement is clearly due to the resistances or sluggishness of the mercurial column.

This instrument consists of a glass globe A and tube A S P, containing a portion of strong sulphuric acid, bent at S to an angle of about 45°; M N a stout scantling of hard wood fixed in a level position, having a slit in it extending nearly from end to end, to allow the lower portion of the tube to slide through it, and having a circular groove extending from F to N, in which the globe A slides; T V a thin board placed at the back of M N; O a round pin on a level with the centre A of the globe (this pin may be placed higher if necessary) upon

which the tube S P slides; e r a scale of temperature attached to the tube S P; E G K a sliding square, the stock E G sliding in a groove formed in the scantling from M to F; m n the thermometrical scale, which is transferred from the scale e r in a way hereafter described; D C the barometrical scale, sliding on the blade G K, and graduated into *equal* parts so as to read off the height of the mercury column balancing the pressure of the atmosphere; Q a piece of thin india-rubber tied over the top of the tube to keep the external air from coming into contact with the sulphuric acid.

The observations of atmospheric pressure are made in the following manner:—The temperature, as indicated by a delicate thermometer, being first noted, the globe A is shifted along the groove F N until the liquid in the tube stands at this temperature, as indicated on the scale e r; the barometrical scale D C is then shifted until its middle point p coincides with the same point of temperature indicated upon the scale m n, and then the point on the scale D C coincident with the level of the liquid in the tube will give the pressure of the atmosphere as measured by a column of mercury.
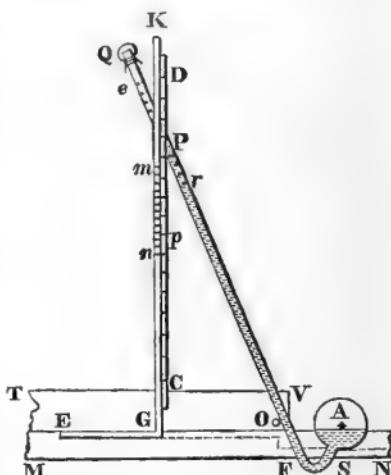
The scales are graduated as follows :—The scale $e\,r$ is formed in exactly the same way as the thermometrical scale of the instrument before described,—with this difference, that the graduations in this case are made when the tube S P is placed at an angle of $45°$ to the horizon. The globe being placed in the frame with its tube inclined at an angle of $45°$, these marks of temperature are transferred to the blade G K of the sliding square and numbered accordingly, thus forming the thermometrical scale $m\,n$. The specific gravity of mercury being taken at 7·5 times that of the sulphuric acid, a unit of 7·5 inches is taken on each side of the central point p of the sliding scale D C, and divided into 100 equal parts; then each of those parts, or ·075 of an inch, will read ·01 of an inch of mercury; and if 29·5 be the mean pressure at which the scale $e\,r$ is made, the point p will be numbered 29·5; and the point coinciding with fifty of these divisions below the point p will correspond to 30 inches of mercury, and so on.

The indications of this instrument are independent of the volume of the globe, as well as of the section of the tube: the ratio $\dfrac{V}{a}$ only affects the range of the scales of temperature $e\,r$ and $m\,n$, which are determined by direct experiment. The superiority of this instrument in point of accuracy over the one previously described, depends on this circumstance, as well as upon the great extent of its range.

It will be readily seen that the level of the liquid in the globe is not at all affected by any change of position. The adjustments for any pressure and temperature of the air being made, it is obvious that the gas in the globe, having the same volume, must also have the same pressure that it had at the same temperature when the tube was in its normal position; hence it follows that the difference in the vertical column of liquid must exactly indicate the change that has taken place in the pressure of the atmosphere. Thus let h_1 be the vertical column of liquid at t_1 temperature, and P_1 pressure of gas in the globe when the tube was in its normal position, that is, when the atmospheric pressure was p; and let h_2 be the vertical column at the same temperature t_1 when the pressure of the atmosphere is p_2; then, as the elastic force of the gas is the same in both cases, we have

$$p + \frac{h_1}{s} = P_1, \text{ and also } p_2 + \frac{h_2}{s} = P_1,$$

$$\therefore\ p + \frac{h_1}{s} = p_2 + \frac{h_2}{s},\ \therefore\ p - p_2 = \frac{1}{s}\,(h_2 - h_1)\,;$$

that is, the difference of atmospheric pressure is exactly proportional to the difference between the vertical columns.

Again, let $k = SP$, the column of liquid in the tube at a given temperature t; $e = SA$; $\theta = \angle EOQ$, the inclination of the tube SP; $h =$ the perpendicular column of liquid above the level of the liquid in the globe; $\theta_1 =$ any other inclination of the tube corresponding to h_1 perpendicular column, the temperature being constant; $c =$ the distance of the level of the liquid in the globe from its centre; $r = OP$;—then we find

$$h = k \sin \theta - e \sin (\theta + 45) + c, \quad \ldots \quad \ldots \quad (8)$$

$$\therefore \; k = \frac{h - h_1 - e\{\sin (\theta + 45) - \sin (\theta_1 + 45)\}}{\sin \theta - \sin \theta_1}, \quad \ldots \quad (9)$$

which gives the length of the liquid column so as to embrace a given range of pressure. For example, let $h - h_1 = 22 \cdot 5$ inches, which is equivalent to 3 inches of mercury; $\theta = 90°$, the greatest angle at which the tube can be placed; $\theta_1 = 15°$, the least angle at which the tube can be conveniently placed; $e = 4$; then we find $k = 30$ inches nearly.

When $\theta = 45°$, equation (8) becomes $h = k \sin 45 - e + c$. This quantity, substituted for h in equation (7), gives the formula for calculating approximately the range of the scale $e\,r$. For example, let $\frac{a}{V} = \frac{1}{100}$, $t = 62°$, $t_1 = 82$, $p = 29 \cdot 5$, $k = 30$, $e = 4$, $c = \cdot 4$; then we find $q_1 = 2 \cdot 6$, or the space of $\cdot 13$ of an inch to each degree of temperature. Supposing, therefore, the instrument to be made at mean temperature ($62°$), and mean pressure ($29 \cdot 5$), the liquid may fill the tube (standing at an angle of $45°$) to within 3 or 4 inches of the top. The globe may be about $2\frac{1}{4}$ inches diameter; length of tube $S\,Q$ about 34 inches; the diameter of the tube about $\cdot 3$ of an inch; and when the tube is held in a vertical position, the liquid should cover the bottom of the globe a little beyond the orifice of the tube leading from the globe.

The temperature t being constant, r and θ variable, the locus of the point P will be expressed by the polar equation

$$\frac{r - k}{1 + \cot \theta} = -e \sin 45.$$

At the limiting angle $\theta = 15°$, we find $OC = 10$ inches, and from the foregoing equation, we find the maximum value of $OG = 24$ inches. The dimensions of M N may therefore be taken as follows: viz. O N about 12 inches, and O M about 26 inches. If the pin O be placed a little higher, then O N should be a little greater, and O M a little less.

Hastings, November 20, 1859.

II. *On the Electric Deportment of the Flame of Alcohol.* *By* W. G. HANKEL*.

NOTWITHSTANDING the numerous researches which, down to the present time, have been published on the electrical deportment of flames and incandescent bodies, our knowledge of most of these phænomena is still very imperfect, and we are for the most part uncertain as to their real causes. The importance, for the theory of electricity, of correctly understanding these phænomena, which are often very remarkable, has induced me for some years past to submit them to a special examination. Today I have the honour to lay before the Physico-Mathematical Class of the Royal Society of Sciences a fifth memoir† of my electrical researches, containing the results I have obtained with respect to the flame of alcohol. I purpose reserving for a later communication the discussion of the phænomena which take place with other flames and with merely glimmering bodies.

The present memoir divides itself into two principal parts. The first comprises the researches on the electric tensions and currents observed in the flame of alcohol; the second treats of the conduction of flames, and more particularly of the so-called unipolar conduction discovered by P. Erman. I believe I am justified in saying that in both respects I have succeeded in tracing back the phænomena to their true cause; and by means of the simplicity thereby given to the results I have obtained, it will be possible to give a brief and intelligible representation of them.

In order to observe and measure the electrical tensions, I made use of my electrometer‡; and for the currents, I used a very sensitive galvanometer with 9960 coils, and an astatic system of two magnetic bars provided with a reflecting apparatus§.

When the flame of an alcohol lamp is put in communication with the earth by means of a wire dipped into the lamp, and a metal is placed in, over, or near the flame, the latter generally becomes electric. When, on the other hand, the metal situated in or above the flame is connected with the earth, and the lamp insulated, the opposite electricity of the same tension is observed on the wire immersed in the lamp.

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* Translated from Poggendorff's *Annalen,* vol. cviii. p. 146, being a paper communicated by the Author to the Royal Scientific Society of Saxony.

† Memoirs of the Royal Scientific Society of Saxony, vol. vii.

‡ Marked A at p. 396 of vol. v. of the Memoirs of the Royal Scientific Society of Saxony.

§ Described at p. 261 of vol. vi. of the same Memoirs.

The nature and intensity of the electrical tension varies with the nature of the metal placed in the lamp, as well as with that of the metal above it.

But the nature and intensity of the electrical tension varies also with the position of the metal above or in the flame. When a thin plate of metal, standing at a considerable height above the lamp, is made to approach the latter, so that it gradually becomes more deeply immersed in the flame and more intensely heated, the electricity first observed varies with this change of position and becomes more negative; or if at first positive, approaches the negative condition more and more. This change may amount to more than the electromotive force of an element formed of zinc, platinum, and alcohol.

No change of electric tension, however, takes place with the change of position when the metal, on approaching or becoming immersed in the flame, is prevented from taking a high temperature by ice being placed upon it; or when, instead of a metal, a jet of water is passed through the flame.

Now in my memoir I give positive proof, by means of measurements, that the electric tensions depend upon the act of combustion only in so far as the gases and vapours of which the flame consists form a conductor, which, like an ordinary liquid conductor, is interposed between the metal situated in or above the flame and the alcohol of the lamp. The mere act of combustion does not produce electricity.

The electricity observed on the metals situated in or above the flame, is nothing more than the tension at the end or pole of a galvanic element having the selected metals for the solid, and the alcohol together with the heated gases of the flame for the liquid conductor.

The variations in the tensions as the metal gradually approaches the flame, are due solely to the increased temperature of the metal, and to the consequent change of its position in the so-called tension-series.

All metals by heating are moved towards the positive end of the tension-series; and when the metals are intensely heated, this change in place may amount to more than the distance between cold zinc and cold platinum, alcohol being employed as a liquid conductor. For equal degrees of temperature, the amount of this change is in all probability not very different in different metals.

If the ends or poles of the galvanic element, formed with alcohol and flame as liquid conductors, upon which we formerly observed free tensions, are joined together, an electric current is produced in consequence of the electromotive power within the circuit, whose direction is determined by the above tensions, and

whose intensity depends also upon the resistance of the whole circuit.

In the second part of my memoir I determine by exact measurements the actions involved in the so-called unipolar conduction. The surfaces of contact of both poles of an element with the flame in connexion with the earth being equal, a small portion of positive electricity remains on the positive pole, whilst the negative pole possesses the whole tension of the element diminished by this small part.

Neither the electricity produced in the pole-surfaces by conducting the flame to the earth, nor the tension conveyed to the flame from one or more galvanic elements intentionally interpolated between it and the earth, change the previous results. The electrical tensions which appear at both poles are equal to the sum of these electricities conveyed to the flame, and of the tensions which would otherwise have existed there.

If the surface of one of the poles is increased, the tension of this pole diminishes, whilst the tension of the opposite pole is increased by the same amount. Exactly the reverse of this takes place when the surface of one of the poles is gradually withdrawn from the lamp.

The case in which an electrical opposition already exists between the metallic plates which serve as poles, required a special explanation. This led to an examination of the tensions at the poles of an unclosed galvanic element, when one metal stands in a liquid opposite to two others which are of different electricities and joined by a conductor. The tension of such an element does not depend merely upon the position of the selected metals in the tension-series, but also upon the resistance of the liquid between the metals, or, to speak more correctly, upon the electrical tension at the point of the tension-curve belonging to the current between the connected metals, where the third metal is immersed.

An elevation of the temperature of the pole-surfaces exercises just as little influence upon the above-mentioned phænomena as does their chemical nature. Jets of water may be substituted for the metallic plates without producing any essential difference. With certain modifications, therefore, the phænomena of unipolar conduction also occur when the poles of a galvanic element are connected, one by means of a metal, and the other by being led to the alcohol of the lamp, with the flame before it is put in communication with the earth.

If, after introducing two equal pole-surfaces of a galvanic element into an insulated flame, the positive one is placed in communication with the earth, the flame receives a negative tension equal to the above-mentioned residue. When the negative

pole is led to the earth, the flame receives a positive electricity equal in amount to the whole tension of the element diminished by the small residue in question.

From what has already been said, it is easy to see in what manner the tensions imparted to the flame vary with the size of the conducting surfaces.

All the phænomena of unipolar conduction may be simply and completely explained by considering the tension-curve of the closed circuit in question. In constructing this tension-curve, it will be remarked that the curve on the metallic conductors, on account of their comparatively small resistance, may be considered as running parallel to the abscissa-axis. The same holds good approximately for the principal part of the flame. On the other hand, owing to the diminution of the cross section, a considerable resistance exists at the pole-surfaces in contact with the flame, and consequently the ordinates of the above curve are there perceptibly altered. Another remarkable result, however, is that a peculiar and very considerable hinderance is opposed to the passage of negative electricity from solid or liquid conductors into the flame, or to the entry of positive from the latter into the conductors, which hinderance is diminished by enlarging the surface of the negative pole. Whilst, then, the tension-curve is nearly parallel to the abscissa-axis along the metallic conductors and the principal part of the flame, it sinks somewhat at the positive pole, and at the negative through the whole remaining portion of the tension of the element.

If, now, the flame is put in connexion with the earth, the metal of the positive pole must receive a positive tension equal to the previous small depression, and the metal of the negative pole a negative tension equal to the depression at this pole.

If the positive pole is joined with the earth, a negative tension is produced in the flame equal to the small depression at the positive pole. Lastly, if the negative pole is joined with the earth, the flame shows a positive tension equal to the greater depression at the negative pole.

The greater resistance which my experiments establish when negative electricity passes into the hot rarefied gases of the flame, also occurs, according to Ed. Becquerel, when an electric current passes through very intensely heated air. I further show how the fact leads to an explanation of the peculiar phænomena observed by Gaugain, when the two opposite currents of an induction apparatus pass through rarefied air.

The greater resistance at the negative pole explains too, lastly, why a current ascends and descends the flame with different degrees of facility.

When Andrews placed a spiral of platinum over the flame of

a gas-lamp and joined it to one of the poles of a voltaic battery, and the metallic tube of the lamp to the other, the current passed more easily from the spiral through the flame to the lamp than in the opposite direction. When a thin plate of platinum is placed above the flame of an alcohol lamp, and one pole of one or more elements is joined to it, and the other to a wire immersed in the alcohol of the lamp, the effects are more precisely the following :—If the thin plate of platinum is situated high above the flame, the current passes more easily downwards through the flame than upwards; if the thin plate of platinum is made to approach the flame, a position will be found in which the current passes equally well in both directions; at a still greater proximity, the flame passes, on the contrary, more easily upwards than downwards. The ratio of the intensities of the currents conducted in opposite directions varies according to the strength of the current.

The explanation of the effects just described must be sought in the peculiar resistance, before mentioned, at the negative pole. At higher positions of the plate above the lamp, the conduction of the flame is more perfect on the side of the wire; at lower positions this conduction is more perfect on the side of the plate; and the current which passes through the flame between the plate and the lamp must always possess greater intensity when the negative pole acquires a relatively better conduction, since its resistance is thereby diminished.

III. *On several New British Minerals.* By R. P. GREG, *Esq.**

SINCE the publication of a 'Manual of the Mineralogy of Great Britain and Ireland,' by Mr. W. G. Lettsom and myself, two years since, several species new to these countries have been noticed, and which were not described in that work; they are anorthite, chrysoberyl, lepidomelane, Beraunite, and Demidoffite; the three former were noticed by Prof. Haughton of Dublin, the two latter by myself.

1. *Anorthite*: occurs with hornblende and syenite at Carlingford Mountain, Co. Down. Analysis by Prof. Haughton :—

Silica	45·87
Alumina	34·73
Lime	17·10
Magnesia	1·55
	99·25

2. *Chrysoberyl*: said to occur in the granite of the Mourne

* Communicated by the Author.

Mountains. (See the Quarterly Journal of the London Geological Society for August 1856.)

3. *Lepidomelane* : a variety of uniaxial mica occurring in small, flat, six-sided crystals, of a black colour, in the granite of Three Rock Mountain, Co. Dublin.

This variety of mica contains an unusually large quantity of the peroxides and protoxides of iron.

4. *Beraunite, Breit* : a variety of Delvauxene, supposed to be a hydrous phosphate of peroxide of iron, resulting from the decomposition of Vivianite. It has recently occurred at Wheal Jane near Truro, in scaly and brittle masses, of a dark brownish-red colour, intimately associated with crystallized and decomposing Vivianite, on eisen-nickelkies. My specimens came from Mr. R. Talling of Lostwithiel.

5. *Demidoffite* : a mineral recently described by Nordenskiold as occurring with green malachite, chrysocolla, and phosphate of copper, at Tagilsk in the Ural Mountains. It occurs at that locality of a pale bluish-green colour, slightly earthy, and coating or encrusting the concentric layers of the mammillated malachite itself: H. 1·5 to 2·0. In the glass tube, yields water with no acid reaction. Composition :—

Silica	31·55
Alumina	0·53
Oxide of copper . .	33·14
Magnesia	3·15
Water	23·03
Phosphoric acid . .	10·22
	101·62

Hitherto it seems that this mineral has been only noticed at the Russian locality; but I have in my collection characteristic specimens, evidently of the same species, from Cumberland, and also from a Cornish locality; as well as from Valparaiso in South America, with malachite and muriate of copper. At both our British localities it occurs with quartzose rock and malachite—the latter, however, not in a mammillated state.

IV. *Extracts from Notes on Electrical Conductivity.*
By J. N. HEARDER, *Electrician, Plymouth**.

IT is generally acknowledged amongst electricians that the term conduction, as applied to metals, implies negative rather than positive qualities; that is to say, all conductors afford a certain amount of resistance, but those are considered

the best which afford the least. Upon this hypothesis, increased transverse sectional area in the same metal diminishes resistance by allowing the transmission of a larger quantity in a given time. The practical determination of the relative conducting capabilities of different metals, or of different samples of the same metal, has generally been accomplished by the comparison of galvanometric or electro-magnetic effects; but I am not aware of any course of experiments which have been undertaken with a view to trace any connexion, or institute any comparison, between the thermal effects of the voltaic current on metals and their conducting powers as thus indicated, or to work out any scale of the conducting powers of metals, based simply upon the thermal effects of the voltaic current upon them.

In 1826 Sir W. S. Harris communicated to the Royal Society the result of a series of experiments with his thermo-electrometer for determining the relative conducting power of metals for the Leyden discharge. His experiments were based upon the assumption that metals are heated by equal discharges of electricity through them, from an electrical jar or battery, in proportion to the resistance which they offer to its passage; hence their relative conducting powers in the scale were considered to be inversely as their thermometric indications. Thus in passing a carefully measured shock through wires of various metals, all of precisely the same diameter and length, stretched through the bulb of an air-thermometer, the relative degrees of heat indicated upon the scale are shown in the following Table, extracted from the Philosophical Transactions of 1827 :—

Metals.	Effects.
Copper	6
Silver	6
Gold	9
Zinc	18
Platinum	30
Iron	30
Tin	36
Lead	72
Brass	18

In the year 1827 I thought of using this thermo-electrometer for determining the relative conducting powers of metals for voltaic electricity, and was pleased, on applying a single pair of plates to it for the first time, to find its indications extremely regular, the fluid rising constantly to the same point at each successive contact, and remaining almost permanent as long as contact was maintained. The instrument, however, appeared to require some few modifications to adapt it more particularly to

voltaic purposes; and after various trials I adopted the form
which I have described in the Philosophical Magazine for May
1857. The metals were all drawn into wires of the same size,
and the same lengths were used in each experiment.

The voltaic batteries which I used were formed upon the prin-
ciple of Dr. Hare's calorimotor and coil batteries. One modifi-
cation consisted of a plate of zinc 6 inches wide and 6 feet in
length, coiled with a similar plate of copper between its convolu-
tions so as to maintain a sphere of half an inch between the cop-
per and zinc, the last coil of copper being made entirely to
enclose the end of the zinc, so that the copper plate was about
six inches longer than the zinc. Both surfaces of each metal
were thus opposed to the action of the others. A second form
consisted of a similar area of zinc and copper cut into plates of
6 inches square, and fastened alternately in grooves in a wooden
frame at a distance of half an inch from each other, the two end
plates being copper. All the zinc plates were united on one side,
and all the copper plates on the other, thus forming a single
pair equal in surface and, as ascertained by experiment, equal in
effect to the coil just described. These batteries were suspended
over a wooden trough by counterbalancing weights, which ad-
mitted of their being immersed either wholly or to any depth in
the acid.

The exciting fluid consisted, by measure, of sulphuric acid 1
part, nitric acid 1 part, and water 120. Stout flexible wires
proceeded from the battery to the thermo-electrometer, and the
battery was plunged in the acid at each experiment and raised
again as soon as it was concluded. The results which I shall
have to detail are rather incomplete in their character, as they
are merely the remains of some scattered memoranda, a great
number of which were mislaid owing to the accident which some
two or three years after deprived me of sight. I am induced,
however, to publish such as I have, since I cannot discover in
my intercourse with electricians that the facts are even now
generally known. I briefly alluded to these experiments on a
former occasion (see paper " On Induction Coil " in the Philo-
sophical Magazine, May 1857, p. 332, note).

I shall forbear to enter into the *rationale* of the phænomena,
but allow the simple facts to be taken for as much as they are
worth.

In my first series of experiments I was met by the curious fact,
that the order in which the metals were heated by the voltaic
arrangements which I employed, was the reverse of that which
took place with the Leyden discharge; that is to say, the best
conductors were the most heated, and the worst the least, as will
be seen by the following Table, in which the length of wire

employed in each experiment was 3·5 inches, and its size about No. 26 wire-gauge. The numbers given are the mean of six experiments, the variation in them not exceeding more than 2 or 3 degrees :—

Metal employed in thermo-electrometer.	Degrees of heat on scale of thermo-electrometer.
Silver	81
Copper	70
Zinc	47
Brass	43
Gold	41
Platinum	41
Tin	39
Iron	35
Lead	26

These experiments were repeated at different times with the same wires and with the same relative results.

Since the best conductors were the most heated in these experiments, it seemed to follow that, with any single metal, larger wires would be heated more than small ones; and this I found to be the case up to the largest size that could be inserted into the electrometer, viz. No. 15 wire-gauge. I regret that a scale of these results with copper wires from No. 15 to No. 36 has been lost, but it showed a curious coincidence between the temperature and the mass of metal. This remarkable inversion of their relative order as compared with the results obtained by the Leyden discharge, whilst it showed a consistency with itself which left no room to attribute it to any error in manipulation, yet seemed to exhibit such an inconsistency with preconceived notions of the laws of electrical conduction, that I was induced to vary the experiments in the following manner.

Instead of introducing the several metallic wires in succession into the bulb of the thermo-electrometer, I used them externally, causing them to form part of the circuit between it and the battery, and employing in the electrometer simply a copper wire much stouter than those under examination. The following Table gives the results, the same wires being used as before :—

Wires in circuit between battery and thermometer.	Degrees on thermometer.
Silver	142
Copper	128
Zinc	93
Brass	92
Gold	70
Tin	61
Platinum	60
Iron	55
Lead	33

Note.—The battery being applied to the electrometer without any of the wires in circuit, the fluid in the stem rose to 176°.

These experiments were repeated at various intervals with wires made indiscriminately from such samples as were at hand, though not with any idea of testing variations in the conducting power of different samples of the same metal; hence the relation between the order of the results was not always the same, as will be seen by the following set of experiments, in which a different set of wires were employed :—

Table I.

Wires in the bulb of the thermometer.	Degrees indicated on the scale.
Copper	180
Zinc.	156
Brass	155
Platinum	128
Tin	126
Iron	110
Lead	104

Table II.

Wires introduced into the circuit.	Degrees on thermo-electrometer containing copper wire.
Circuit completed without wire .	150
Copper	128
Zinc	93
Brass	92
Tin	61
Platinum	60
Iron	55
Lead	33

The discrepancy in these results, though perplexing at the time, is now easily accounted for, when it is considered that even samples of copper wire vary as much in their conducting power as 50 to 120.

On employing two electrometers in consecutive circuit, in one of which was inserted a stout copper wire, and in the other the various metals in succession, it was curious to observe the fluid stand.highest in both when the best conductors were used, and lowest with the worst; whilst on employing a similar arrangement for transmitting the charge of an electrical battery the order was reversed, each electrometer giving the highest when the other gave the lowest results.

Whilst experimenting on one occasion with the various wires externally to the electrometer, I had the curiosity to bring my finger in contact with the wire to ascertain its temperature. I

remarked that every time I touched it the fluid in the electro-meter rose, indicating an increase of temperature, and implying also an increase of conducting power in the metal thus touched. I found that this was owing to a reduction of its temperature; for on subsequently moistening it with ether, water, &c., or by blowing upon it, the fluid rose in the electrometer as the tem-perature was reduced, whilst the application of a spirit-lamp to increase the temperature of the wire produced a corresponding fall in the thermometer. Two electrometers were subsequently employed in circuit, the same current passing consecutively through them. To one of the electrometers a second battery was applied. The result was an increase of temperature of the included wire; and I discovered that, by raising or lowering the second battery so as to gradually increase or diminish the tem-perature of one of the wires, the fluid as it rose and fell in that electrometer gave rise to a reverse motion of the fluid in the other, so that as one rose the other fell, and *vice versâ*.

Although these experiments were made more than thirty years since, I am induced to believe that they may still appear novel to some, since, in a conversation a short time since with one of the first electricians of the day, he would scarcely credit them, alleging that they were contrary to all our experience; they must, however, be taken as indicating only the results due to the peculiar arrangements and conditions herein described.

V. *Illustrations of the Dynamical Theory of Gases.*—Part I. *On the Motions and Collisions of Perfectly Elastic Spheres. By* J. C. MAXWELL, *M.A., Professor of Natural Philosophy in Marischal College and University of Aberdeen*[*].

SO many of the properties of matter, especially when in the gaseous form, can be deduced from the hypothesis that their minute parts are in rapid motion, the velocity increasing with the temperature, that the precise nature of this motion becomes a subject of rational curiosity. Daniel Bernouilli, Hera-path, Joule, Krönig, Clausius, &c. have shown that the relations between pressure, temperature, and density in a perfect gas can be explained by supposing the particles to move with uniform velocity in straight lines, striking against the sides of the con-taining vessel and thus producing pressure. It is not necessary to suppose each particle to travel to any great distance in the same straight line; for the effect in producing pressure will be the same if the particles strike against each other; so that the straight line described may be very short. M. Clausius has de-termined the mean length of path in terms of the average distance

of the particles, and the distance between the centres of two particles when collision takes place. We have at present no means of ascertaining either of these distances; but certain phænomena, such as the internal friction of gases, the conduction of heat through a gas, and the diffusion of one gas through another, seem to indicate the possibility of determining accurately the mean length of path which a particle describes between two successive collisions. In order to lay the foundation of such investigations on strict mechanical principles, I shall demonstrate the laws of motion of an indefinite number of small, hard, and perfectly elastic spheres acting on one another only during impact.

If the properties of such a system of bodies are found to correspond to those of gases, an important physical analogy will be established, which may lead to more accurate knowledge of the properties of matter. If experiments on gases are inconsistent with the hypothesis of these propositions, then our theory, though consistent with itself, is proved to be incapable of explaining the phænomena of gases. In either case it is necessary to follow out the consequences of the hypothesis.

Instead of saying that the particles are hard, spherical, and elastic, we may if we please say that the particles are centres of force, of which the action is insensible except at a certain small distance, when it suddenly appears as a repulsive force of very great intensity. It is evident that either assumption will lead to the same results. For the sake of avoiding the repetition of a long phrase about these repulsive forces, I shall proceed upon the assumption of perfectly elastic spherical bodies. If we suppose those aggregate molecules which move together to have a bounding surface which is not spherical, then the rotatory motion of the system will store up a certain proportion of the whole *vis viva*, as has been shown by Clausius, and in this way we may account for the value of the specific heat being greater than on the more simple hypothesis.

On the Motion and Collision of Perfectly Elastic Spheres.

Prop. I. Two spheres moving in opposite directions with velocities inversely as their masses strike one another; to determine their motions after impact.

Let P and Q be the position of the centres at impact; A P, B Q the directions and magnitudes of the velocities before impact; P *a*, Q *b* the same after impact; then, resolving the velocities parallel and perpendicular to P Q the line of centres, we find that the velocities parallel to the line of centres are
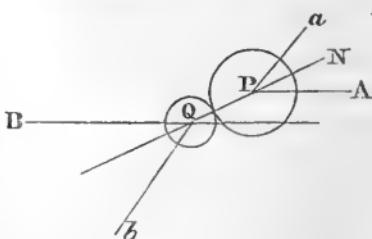
exactly reversed, while those perpendicular to that line are unchanged. Compounding these velocities again, we find that the velocity of each ball is the same before and after impact, and that the directions before and after impact lie in the same plane with the line of centres, and make equal angles with it.

Prop. II. To find the probability of the direction of the velocity after impact lying between given limits.

In order that a collision may take place, the line of motion of one of the balls must pass the centre of the other at a distance less than the sum of their radii; that is, it must pass through a circle whose centre is that of the other ball, and radius (s) the sum of the radii of the balls. Within this circle every position is equally probable, and therefore the probability of the distance from the centre being between r and $r + dr$ is

$$\frac{2r\,dr}{s^2}.$$

Now let ϕ be the angle APa between the original direction and the direction after impact, then APN $= \frac{1}{2}\phi$, and $r = s \sin \frac{1}{2}\phi$, and the probability becomes

$$\tfrac{1}{2} \sin \phi \, d\phi.$$

The area of a spherical zone between the angles of polar distance ϕ and $\phi + d\phi$ is

$$2\pi \sin \phi \, d\phi \,;$$

therefore if ω be any small area on the surface of a sphere, radius unity, the probability of the direction of rebound passing through this area is

$$\frac{\omega}{4\pi};$$

so that the probability is independent of ϕ, that is, all directions of rebound are equally likely.

Prop. III. Given the direction and magnitude of the velocities of two spheres before impact, and the line of centres at impact; to find the velocities after impact.

Let O A, O B represent the velocities before impact, so that if there had been no action between the bodies they would have

been at A and B at the end of a second. Join A B, and let G be their centre of gravity, the position of which is not affected by their mutual action. Draw G N parallel to the line of centres at impact (not necessarily in the plane A O B). Draw a G b in the
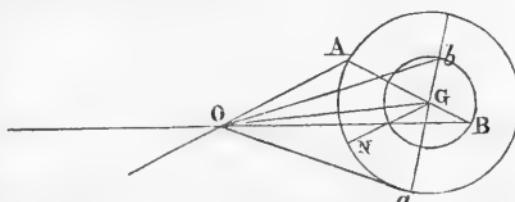
plane AGN, making N G a = N G A, and G a = G A and G b = G B; then by Prop. I. G a and G b will be the velocities relative to G ; and compounding these with O G, we have O a and O b for the true velocities after impact.

By Prop. II. all directions of the line a G b are equally probable. It appears therefore that the velocity after impact is compounded of the velocity of the centre of gravity, and of a velocity equal to the velocity of the sphere relative to the centre of gravity, which may with equal probability be in any direction whatever.

If a great many equal spherical particles were in motion in a perfectly elastic vessel, collisions would take place among the particles, and their velocities would be altered at every collision ; so that after a certain time the *vis viva* will be divided among the particles according to some regular law, the average number of particles whose velocity lies between certain limits being ascertainable, though the velocity of each particle changes at every collision.

Prop. IV. To find the average number of particles whose velocities lie between given limits, after a great number of collisions among a great number of equal particles.

Let N be the whole number of particles. Let x, y, z be the components of the velocity of each particle in three rectangular directions, and let the number of particles for which x lies between x and $x + dx$ be N$f(x)dx$, where $f(x)$ is a function of x to be determined.

The number of particles for which y lies between y and $y + dy$ will be N$f(y)dy$; and the number for which z lies between z and $z + dz$ will be N$f(z)dz$, where f always stands for the same function.

Now the existence of the velocity x does not in any way affect that of the velocities y or z, since these are all at right angles to each other and independent, so that the number of particles whose velocity lies between x and $x + dx$, and also between y and $y + dy$, and also between z and $z + dz$, is

$$N f(x) f(y) f(z) dx \, dy \, dz.$$

If we suppose the N particles to start from the origin at the same instant, then this will be the number in the element of volume ($dx \, dy \, dz$) after unit of time, and the number referred to unit of volume will be

$$N f(x) f(y) f(z).$$

But the directions of the coordinates are perfectly arbitrary, and therefore this number must depend on the distance from the origin alone, that is

$$f(x) f(y) f(z) = \phi(x^2 + y^2 + z^2).$$

Solving this functional equation, we find

$$f(x) = Ce^{Ax^2}, \quad \phi(r^2) = C^3 e^{Ar^2}.$$

If we make A positive, the number of particles will increase with the velocity, and we should find the whole number of particles infinite. We therefore make A negative and equal to $-\dfrac{1}{\alpha^2}$, so that the number between x and $x + dx$ is

$$NCe^{-\frac{x^2}{\alpha^2}} dx.$$

Integrating from $x = -\infty$ to $x = +\infty$, we find the whole number of particles,

$$NC\sqrt{\pi}\alpha = N, \quad \therefore C = \frac{1}{\alpha\sqrt{\pi}},$$

$f(x)$ is therefore

$$\frac{1}{\alpha\sqrt{\pi}} e^{-\frac{x^2}{\alpha^2}}.$$

Whence we may draw the following conclusions :—

1st. The number of particles whose velocity, resolved in a certain direction, lies between x and $x + dx$ is

$$N \frac{1}{\alpha\sqrt{\pi}} e^{-\frac{x^2}{\alpha^2}} dx. \quad \ldots \quad (1)$$

2nd. The number whose actual velocity lies between v and $v + dv$ is

$$N \frac{4}{\alpha^3\sqrt{\pi}} v^2 e^{-\frac{v^2}{\alpha^2}} dv. \quad \ldots \quad (2)$$

3rd. To find the mean value of v, add the velocities of all the particles together and divide by the number of particles; the result is

$$\text{mean velocity} = \frac{2\alpha}{\sqrt{\pi}}. \quad \ldots \quad (3)$$

4th. To find the mean value of v^2, add all the values together and divide by N,

$$\text{mean value of } v^2 = \tfrac{3}{2}\alpha^2. \quad \ldots \quad (4)$$

This is greater than the square of the mean velocity, as it ought to be.

It appears from this proposition that the velocities are distributed among the particles according to the same law as the errors are distributed among the observations in the theory of the "method of least squares." The velocities range from 0 to ∞, but the number of those having great velocities is comparatively small. In addition to these velocities, which are in all directions equally, there may be a general motion of translation

of the entire system of particles which must be compounded with the motion of the particles relatively to one another. We may call the one the motion of translation, and the other the motion of agitation.

Prop. V. Two systems of particles move each according to the law stated in Prop. IV.; to find the number of pairs of particles, one of each system, whose relative velocity lies between given limits.

Let there be N particles of the first system, and N$'$ of the second, then NN$'$ is the whole number of such pairs. Let us consider the velocities in the direction of x only; then by Prop. IV. the number of the first kind, whose velocities are between x and $x + dx$, is

$$\mathrm{N} \frac{1}{\alpha \sqrt{\pi}} \epsilon^{-\frac{x^2}{\alpha^2}} dx,$$

The number of the second kind, whose velocity is between $x + y$ and $x + y + dy$, is

$$\mathrm{N}' \frac{1}{\beta \sqrt{\pi}} \epsilon^{-\frac{(x+y)^2}{\beta^2}} dy,$$

where β is the value of α for the second system.

The number of pairs which fulfil both conditions is

$$\mathrm{NN}' \frac{1}{\alpha\beta\pi} \epsilon^{-\left(\frac{x^2}{\alpha^2} + \frac{(x+y)^2}{\beta^2}\right)} dx\, dy.$$

Now x may have any value from $-\infty$ to $+\infty$ consistently with the difference of velocities being between y and $y + dy$; therefore integrating between these limits, we find

$$\mathrm{NN}' \frac{1}{\sqrt{\alpha^2 + \beta^2} \sqrt{\pi}} \epsilon^{-\frac{y^2}{\alpha^2 + \beta^2}} dy \quad . \quad . \quad . \quad . \quad (5)$$

for the whole number of pairs whose difference of velocity lies between y and $y + dy$.

This expression, which is of the same form with (1) if we put NN$'$ for N, $\alpha^2 + \beta^2$ for α^2, and y for x, shows that the distribution of relative velocities is regulated by the same law as that of the velocities themselves, and that the mean relative velocity is the square root of the sum of the squares of the mean velocities of the two systems.

Since the direction of motion of every particle in one of the systems may be reversed without changing the distribution of velocities, it follows that the velocities compounded of the velocities of two particles, one in each system, are distributed according to the same formula (5) as the relative velocities.

Prop. VI. Two systems of particles move in the same vessel;

to prove that the mean *vis viva* of each particle will become the same in the two systems.

Let P be the mass of each particle of the first system, Q that of each particle of the second.   Let p, q be the mean velocities in the two systems before impact, and let p', q' be the mean velocities after one impact. Let A O $= p$ and O B $= q$, and let A O B be a right angle ; then, by Prop. V., A B will be the mean relative velocity, O G will be the mean velocity of centre of gravity ; and drawing a G b at right angles to O G, and making a G $=$ A G and b G $=$ B G, then O a will be the mean velocity of P after impact, compounded of O G and G a, and O b will be that of Q after impact.

Now

$$AB = \sqrt{p^2+q^2}, \ AG = \frac{Q}{P+Q}\sqrt{p^2+q^2}, \ BG = \frac{P}{P+Q}\sqrt{p^2+q^2},$$

$$OG = \frac{\sqrt{P^2 p^2 + Q^2 q^2}}{P+Q},$$

therefore

$$p' = Oa = \frac{\sqrt{Q^2(p^2+q^2)+P^2 p^2 + Q^2 q^2}}{P+Q}$$

and

$$q' = Ob = \frac{\sqrt{P^2(p^2+q^2)+P^2 p^2 + Q^2 q^2}}{P+Q},$$

and

$$Pp'^2 - Qq'^2 = \left(\frac{P-Q}{P+Q}\right)^2 (Pp^2 - Qq^2). \quad . \quad . \quad . \quad (6)$$

It appears therefore that the quantity $Pp^2 - Qq^2$ is diminished at every impact in the same ratio, so that after many impacts it will vanish, and then

$$Pp^2 = Qq^2.$$

Now the mean *vis viva* is $\frac{3}{2}Pa^2 = \frac{3\pi}{8}Pp^2$ for P, and $\frac{3\pi}{8}Qq^2$ for Q; and it is manifest that these quantities will be equal when $Pp^2 = Qq^2$.

If any number of different kinds of particles, having masses P, Q, R, and velocities p, q, r respectively, move in the same vessel, then after many impacts

$$Pp^2 = Qq^2 = Rr^2, \&c. \quad . \quad . \quad . \quad . \quad (7)$$

Prop. VII. A particle moves with velocity r relatively to a number of particles of which there are N in unit of volume ; to
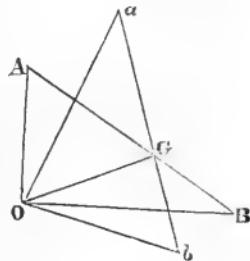
find the number of these which it approaches within a distance s in unit of time.

If we describe a tubular surface of which the axis is the path of the particle, and the radius the distance s, the content of this surface generated in unit of time will be $\pi r s^2$, and the number of particles included in it will be

$$N\pi r s^2, \qquad \ldots \qquad (8)$$

which is the number of particles to which the moving particle approaches within a distance s.

Prop. VIII. A particle moves with velocity v in a system moving according to the law of Prop. IV.; to find the number of particles which have a velocity relative to the moving particle between r and $r + dr$.

Let u be the actual velocity of a particle of the system, v that of the original particle, and r their relative velocity, and θ the angle between v and r, then

$$u^2 = v^2 + r^2 - 2vr \cos \theta.$$

If we suppose, as in Prop. IV., all the particles to start from the origin at once, then after unit of time the "density" or number of particles to unit of volume at distance u will be

$$N \frac{1}{\alpha^3 \pi^{\frac{3}{2}}} e^{-\frac{u^2}{\alpha^2}}.$$

From this we have to deduce the number of particles in a shell whose centre is at distance v, radius $= r$, and thickness $= dr$,

$$N \frac{1}{\alpha \sqrt{\pi}} \frac{r}{v} \left\{ e^{-\frac{(r-v)^2}{\alpha^2}} - e^{-\frac{(r+v)^2}{\alpha^2}} \right\} dr, \qquad \ldots \qquad (9)$$

which is the number required.

Cor. It is evident that if we integrate this expression from $r = 0$ to $r = \infty$, we ought to get the whole number of particles $= N$, whence the following mathematical result,

$$\int_0^\infty dx \cdot x \left(e^{-\frac{(x-a)^2}{\alpha^2}} - e^{-\frac{(x+a)^2}{\alpha^2}} \right) = \sqrt{\pi} a \alpha. \qquad \ldots \qquad (10)$$

Prop. IX. Two sets of particles move as in Prop. V.; to find the number of pairs which approach within a distance s in unit of time.

The number of the second kind which have a velocity between v and $v + dv$ is

$$N' \frac{4}{\beta^3 \sqrt{\pi}} v^2 e^{-\frac{v^2}{\beta^2}} dv = n'.$$

The number of the first kind whose velocity relative to these is

between r and $r + dr$ is

$$\mathrm{N} \frac{1}{\alpha \sqrt{\pi}} \frac{r}{v} (e^{-\frac{(r-v)^2}{\alpha^2}} - e^{-\frac{(r+v)^2}{\alpha^2}}) dr = n,$$

and the number of pairs which approach within distance s in unit of time is

$$n\, n' \pi r s^2,$$

$$= \mathrm{N}\mathrm{N}' \frac{4}{\alpha \beta^3} s^2 r^2 v\, e^{-\frac{v^2}{\beta}} \left\{ e^{-\frac{(v-r)^2}{\alpha^2}} - e^{-\frac{(v+r)^2}{\alpha^2}} \right\} dr\, dv.$$

By the last proposition we are able to integrate with respect to v, and get

$$\mathrm{N}\mathrm{N}' \frac{4 \sqrt{\pi}}{(\alpha^2 + \beta^2)^{\frac{3}{2}}} s^2 r^3 e^{-\frac{r^2}{\alpha^2 + \beta^2}} dr.$$

Integrating this again from $r = 0$ to $r = \infty$,

$$2\mathrm{N}\mathrm{N}' \sqrt{\pi} \sqrt{\alpha^2 + \beta^2} s^2 \quad . \quad . \quad . \quad . \quad (11)$$

is the number of collisions in unit of time which take place in unit of volume between particles of different kinds, s being the distance of centres at collision. The number of collisions between two particles of the first kind, s_1 being the striking distance, is

$$2\mathrm{N}^2 \sqrt{\pi} \sqrt{2\alpha^2} s_1^2 ;$$

and for the second system it is

$$2\mathrm{N}'^2 \sqrt{\pi} \sqrt{2\beta^2} s_2^2.$$

The mean velocities in the two systems are $\dfrac{2\alpha}{\sqrt{\pi}}$ and $\dfrac{2\beta}{\sqrt{\pi}}$; so that if l_1 and l_2 be the mean distances travelled by particles of the first and second systems between each collision, then

$$\frac{1}{l_1} = \pi \mathrm{N}_1 \sqrt{2}\, s_1^2 + \pi \mathrm{N}_2 \frac{\sqrt{\alpha^2 + \beta^2}}{\alpha} s^2,$$

$$\frac{1}{l_2} = \pi \mathrm{N}_1 \frac{\sqrt{\alpha^2 + \beta^2}}{\beta} s^2 + \pi \mathrm{N}_2 \sqrt{2}\, s_2^2.$$

Prop. X. To find the probability of a particle reaching a given distance before striking any other.

Let us suppose that the probability of a particle being stopped while passing through a distance dx, is αdx ; that is, if N particles arrived at a distance x, $\mathrm{N}\alpha dx$ of them would be stopped before getting to a distance $x + dx$. Putting this mathematically,

$$\frac{d\mathrm{N}}{dx} = -\mathrm{N}\alpha, \text{ or } \mathrm{N} = \mathrm{C}e^{-\alpha x}.$$

Putting N=1 when $x=0$, we find $e^{-\alpha x}$ for the probability of a particle not striking another before it reaches a distance x.

The *mean distance* travelled by each particle before striking is $\frac{1}{\alpha}=l$. The probability of a particle reaching a distance $=nl$ without being struck is e^{-n}. (See a paper by M. Clausius, Philosophical Magazine, February 1859.)

If all the particles are at rest but one, then the value of α is

$$\alpha=\pi s^2 N,$$

where s is the distance between the centres at collision, and N is the number of particles in unit of volume. If v be the velocity of the moving particle relatively to the rest, then the number of collisions in unit of time will be

$$v\pi s^2 N ;$$

and if v_1 be the actual velocity, then the number will be $v_1\alpha$; therefore

$$\alpha=\frac{v}{v_1}\pi s^2 N,$$

where v_1 is the actual velocity of the striking particle, and v its velocity relatively to those it strikes. If v_2 be the actual velocity of the other particles, then $v=\sqrt{v_1{}^2+v_2{}^2}$. If $v_1=v_2$, then $v=\sqrt{2}v_1$, and

$$\alpha=\sqrt{2}\pi s^2 N.$$

Note.—M. Clausius makes $\alpha=\frac{4}{3}\pi s^2 N$.

Prop. XI. In a mixture of particles of two different kinds, to find the mean path of each particle.

Let there be N_1 of the first, and N_2 of the second in unit of volume. Let s_1 be the distance of centres for a collision between two particles of the first set, s_2 for the second set, and s' for collision between one of each kind. Let v_1 and v_2 be the coefficients of velocity, $M_1 M_2$ the mass of each particle.

The probability of a particle M_1 not being struck till after reaching a distance x_1 by another particle of the same kind is

$$e^{-\sqrt{2}\pi s_1{}^2 N_1 x}.$$

The probability of not being struck by a particle of the other kind in the same distance is

$$e^{-\sqrt{1+\frac{v_2{}^2}{v_1{}^2}}\pi s'^2 N_2 x}.$$

Therefore the probability of not being struck by any particle before reaching a distance x is

$$e^{-\pi\left(\sqrt{2}s_1{}^2 N_1+\sqrt{1+\frac{v_2{}^2}{v_1{}^2}}s'^2 N_2\right)x} ;$$

and if l_1 be the *mean distance* for a particle of the first kind,

$$\frac{1}{l_1} = \sqrt{2}\pi s_1^2 N_1 + \pi \sqrt{1 + \frac{v_2^2}{v_1^2}} s'^2 N_2. \quad . \quad . \quad (12)$$

Similarly, if l_2 be the mean distance for a particle of the second kind,

$$\frac{1}{l_2} = \sqrt{2}\pi s_2^2 N_2 + \pi \sqrt{1 + \frac{v_1^2}{v_2^2}} s'^2 N_1. \quad . \quad . \quad (13)$$

The mean density of the particles of the first kind is $N_1 M_1 = \rho_1$, and that of the second $N_2 M_2 = \rho_2$. If we put

$$A = \sqrt{2}\frac{\pi s_1^2}{M_1}, \quad B = \pi \sqrt{1 + \frac{v_2^2}{v_1^2}} \frac{s'^2}{M_2}, \quad C = \pi \sqrt{1 + \frac{v_1^2}{v_2^2}} \frac{s'^2}{M_1},$$

$$D = \sqrt{2}\frac{\pi s_2}{M_2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

$$\frac{1}{l_1} = A\rho_1 + B\rho_2, \quad \frac{1}{l_2} = C\rho_1 + D\rho^2, \quad . \quad . \quad (15)$$

and

$$\frac{B}{C} = \frac{M_1 v_2}{M_2 v_1} = \frac{v_2^3}{v_1^3}. \quad . \quad . \quad . \quad . \quad . \quad (16)$$

Prop. XII. To find the pressure on unit of area of the side of the vessel due to the impact of the particles upon it.

Let N = number of particles in unit of volume;

    M = mass of each particle;

    v = velocity of each particle;

    l = mean path of each particle;

then the number of particles in unit of area of a stratum dz thick is

$$N dz. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)$$

The number of collisions of these particles in unit of time is

$$N dz \frac{v}{l}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

The number of particles, which after collision reach a distance between nl and $(n+dn)l$, is

$$N \frac{v}{l} e^{-n} dz \, dn. \quad . \quad . \quad . \quad . \quad . \quad (19)$$

The proportion of these which strike on unit of area at distance z is

$$\frac{nl - z}{2nl}; \quad . \quad . \quad . \quad . \quad . \quad . \quad (20)$$

the mean velocity of these in the direction of z is

$$v \frac{nl + z}{2nl}. \quad . \quad . \quad . \quad . \quad . \quad (21)$$

Multiplying together (19), (20), and (21), and M, we find the momentum at impact

$$\mathrm{MN} \frac{v^2}{4n^2l^3}(n^2l^2 - z^2)e^{-n}\,dz\,dn.$$

Integrating with respect to z from 0 to nl, we get

$$\tfrac{1}{6}\mathrm{MN}v^2\,ne^{-n}\,dn.$$

Integrating with respect to n from 0 to ∞, we get

$$\tfrac{1}{6}\mathrm{MN}v^2$$

for the momentum in the direction of z of the striking particles; the momentum of the particles after impact is the same, but in the opposite direction; so that the whole pressure on unit of area is twice this quantity, or

$$p = \tfrac{1}{3}\mathrm{MN}v^2. \quad \ldots \quad \ldots \quad (22)$$

This value of p is independent of l the length of path. In applying this result to the theory of gases, we put $\mathrm{MN} = \rho$, and $v^2 = 3k$, and then

$$p = k\rho,$$

which is Boyle and Mariotte's law. By (4) we have

$$v^2 = \tfrac{3}{2}\alpha^2, \quad \therefore \ \alpha^2 = 2k. \quad \ldots \quad \ldots \quad (23)$$

We have seen that, on the hypothesis of elastic particles moving in straight lines, the pressure of a gas can be explained by the assumption that the square of the velocity is proportional directly to the absolute temperature, and inversely to the specific gravity of the gas at constant temperature, so that at the same pressure and temperature the value of $\mathrm{N}Mv^2$ is the same for all gases. But we found in Prop. VI. that when two sets of particles communicate agitation to one another, the value of Mv^2 is the same in each. From this it appears that N, the number of particles in unit of volume, is the same for all gases at the same pressure and temperature. This result agrees with the chemical law, that equal volumes of gases are chemically equivalent.

We have next to determine the value of l, the mean length of the path of a particle between consecutive collisions. The most direct method of doing this depends upon the fact, that when different strata of a gas slide upon one another with different velocities, they act upon one another with a tangential force tending to prevent this sliding, and similar in its results to the friction between two solid surfaces sliding over each other in the same way. The explanation of gaseous friction, according to our hypothesis, is, that particles having the mean velocity of translation belonging to one layer of the gas, pass out of it into another layer having a different velocity of translation; and by striking against the particles of the second layer, exert upon it

a tangential force which constitutes the internal friction of the gas. The whole friction between two portions of gas separated by a plane surface, depends upon the total action between all the layers on the one side of that surface upon all the layers on the other side.

Prop. XIII. To find the internal friction in a system of moving particles.

Let the system be divided into layers parallel to the plane of xy, and let the motion of translation of each layer be u in the direction of x, and let $u = A + Bz$. We have to consider the mutual action between the layers on the positive and negative sides of the plane xy. Let us first determine the action between two layers dz and dz', at distances z and $-z'$ on opposite sides of this plane, each unit of area. The number of particles which, starting from dz in unit of time, reach a distance between nl and $(n+dn)l$ is by (19),

$$N \frac{v}{l} e^{-n} \, dz \, dn.$$

The number of these which have the ends of their paths in the layer dz' is

$$N \frac{v}{2nl^2} e^{-n} \, dz \, dz' \, dn.$$

The mean velocity in the direction of x which each of these has before impact is $A + Bz$, and after impact $A + Bz'$; and its mass is M, so that a mean momentum $= MB(z - z')$ is communicated by each particle. The whole action due to these collisions is therefore

$$NMB \frac{v}{2nl^2} (z - z') e^{-n} \, dz \, dz' \, dn.$$

We must first integrate with respect to z' between $z' = 0$ and $z' = z - nl$; this gives

$$\tfrac{1}{2} NMB \frac{v}{2nl^2} (n^2 l^2 - z^2) e^{-n} \, dz \, dn$$

for the action between the layer dz and all the layers below the plane xy. Then integrate from $z = 0$ to $z = nl$,

$$\tfrac{1}{6} MNBlvn^2 e^{-n} \, dn.$$

Integrate from $n = 0$ to $n = \infty$, and we find the whole friction between unit of area above and below the plane to be

$$F = \tfrac{1}{3} MNlvB = \tfrac{1}{3} \rho lv \frac{du}{dz} = \mu \frac{du}{dz},$$

where μ is the ordinary coefficient of internal friction,

$$\mu = \tfrac{1}{3} \rho lv = \frac{1}{3\sqrt{2}} \frac{Mv}{\pi s^2}, \quad \ldots \quad (24)$$

where ρ is the density, l the mean length of path of a particle,

and v the mean velocity $v = \dfrac{2\alpha}{\sqrt{\pi}} = 2\sqrt{\dfrac{2k}{\pi}}$,

$$l = \tfrac{3}{2}\frac{\mu}{\rho}\sqrt{\frac{\pi}{2k}} \quad . \quad . \quad . \quad . \quad . \quad (25)$$

Now Professor Stokes finds by experiments on air,

$$\sqrt{\frac{\mu}{\rho}} = \cdot 116.$$

If we suppose $\sqrt{k} = 930$ feet per second for air at 60°, and therefore the mean velocity $v = 1505$ feet per second, then the value of l, the mean distance travelled over by a particle between consecutive collisions, $= \frac{1}{447000}$th of an inch, and each particle makes 8,077,200,000 collisions per second.

A remarkable result here presented to us in equation (24), is that if this explanation of gaseous friction be true, the coefficient of friction is independent of the density. Such a consequence of a mathematical theory is very startling, and the only experiment I have met with on the subject does not seem to confirm it. We must next compare our theory with what is known of the diffusion of gases, and the conduction of heat through a gas.

[To be continued.]

VI. *On the different States of Silicic Acid.* By M. H. Rose[*].

NUMEROUS determinations of the density of silicic acid, and especially those of Count Schaffgotsch, prove that there exist two distinct modifications of this acid, one of which has a density of 2·6, whilst in the other the density rises to 2·2, or 2·3. The first is always crystallized, or more or less crystal-line, the second always amorphous.

Crystallized silica is found not only in rock-crystal, quartz, amethyst, sandstone, and quartzose sand, but also in a great number of the varieties of silica, in appearance compact, but really formed of an aggregation of crystalline particles, as their property of polarizing light proves—such are chalcedony, chrysoprase, jasper, flint, and certain siliceous woods. Some of these varieties may contain traces of water or foreign matter, which make their density vary a little, without, however, causing the same to fall below 2·6.

The chemical and physical properties of all these substances are exactly the same. If crystallized quartz seems to resist some-

what more the action of chemical agents, such as hydrofluoric acid or caustic potash, it is easy to see that this slight difference depends only upon the great cohesion which a more perfect crystallization determines. In fact, rock-crystal and silica, both reduced to an extremely fine powder, do not present any difference. It is almost beyond doubt that the varieties of compact and crystalline silica, such as flint, agate, and siliceous woods, have been formed in the humid way. The preservation of the ligneous structure in these last, the presence of infusoria in flint, signalized by Ehrenberg, the transformation of a great number of fossils into flint, are sufficient proofs. A great number of facts also prove that rock-crystal and ordinary quartz, which have the same density, can only have been formed in the humid way, or at least by the influence of water.

We have succeeded by various modes of treatment, but only in the humid way, in obtaining crystallized silicic acid in the form of quartz. M. de Sénarmont has obtained this result by heating, in a closed vessel at 200° or 300°, a solution of silicic acid in water acidulated by carbonic or hydrochloric acid.

M. Daubrée has obtained silica in a crystalline state by the action of the vapour of water on chloride or fluoride of silicium at a red heat; afterwards he obtained it in distinct crystals by the action of water upon glass, under the influence of an elevated temperature and high pressure.

The frequent association, in several formations, of crystallized quartz with silica in a compact crystalline state, also shows that it must have been formed in the humid way.

On the contrary, notwithstanding several attempts, we have never succeeded in obtaining by means of heat crystallized or compact crystalline silica.

But the strongest argument against the supposition that quartz has passed through the state of igneous fusion before its crystallization, is found in the fact that quartz passes by fusion into the amorphous condition whose density is 2·2.

. The fusion of quartz has often been effected, by Davy, Clarke, Stromeyer, and Marcet, more recently also by Gaudin and Deville. In every case, after this fusion, silica is completely amorphous and vitreous, its density being 2·2. If the objection is raised, that fused quartz might have passed into a crystalline state by an extremely slow cooling, one may reply that it is impossible that a granitic mass can have cooled with the same slowness throughout its whole extent; there must necessarily have been portions exposed to a more rapid cooling; so that here and there the amorphous modification of quartz ought to be found. But its presence has never been detected in rocks. It has sometimes been supposed that the crystallization might have been

produced by the action of a very elevated temperature below the point of fusion, as occurs sometimes in the devitrification and crystallization of glass. Some experiments of Prof. Rose show the equal impossibility of sustaining this view. These experiments were made in a porcelain kiln, where the temperature rises to and remains during eighteen hours at about 2000° Centigrade (according to M. Elsner), after which the substances thus heated cool very slowly.

A crystal of quartz perfectly transparent did not undergo any alteration by this test; its density of 2·651 was afterwards found to be 2·650. A crystal of the same quartz, the angularity of which was intact, but the inferior extremity of which was cracked, did not sustain any alteration in the intact portion, but became whitish and brittle in the part which was already fissured. Those portions which had become opaline, possessed a density of 2·613, which indicated the passage of the quartz to a modification less dense. Lastly, a crystal of very pure quartz was submitted to the same test, after having been previously reduced to a very fine powder. This powder subsided considerably, without however cohering, and its density fell to 2·394. It was exposed a second time in the furnace, and the density became 2·329.

A fragment of blackish flint, density 2·591, was submitted to the same process. Without changing its form, it became white and capable of very easy pulverization. The density of an entire fragment was found to be 2·218; that of the fine powder 2·237.

It results from these experiments that crystallized silica passes gradually into the amorphous modification when it is exposed to an elevated temperature below its point of fusion. They prove also that a more perfect crystalline state opposes a greater resistance to this transformation, as it does to the action of solvents.

The amorphous modification is not produced merely by the action of great heat upon crystallized silica; it is obtained also on fusing the same with an alkali and afterwards decomposing the alkaline silicate by an acid, and generally whenever a natural or artificial silicate is decomposed by an acid.

Here the author signalizes various phænomena which are observed during this decomposition. Sometimes the silica remains in solution, sometimes it becomes separated in the pulverulent state, sometimes it determines the coagulation of the liquor into a gelatinous mass. He also signalizes the influence of calcination on certain silicates,—sometimes rendering undecomposable by acids those which were previously decomposed with facility, sometimes, on the contrary, facilitating their decomposition. These facts have led certain chemists to suppose the existence of

two distinct modifications of silicic acid in silicates. Whatever the value of this hypothesis may be, it results from the experiments of M. Rose, that, whatever may be the condition in which silica presents itself at the time of the decomposition of the silicates by acids, it possesses always the same properties. Its density is about 2·2, and rises to 2·3 by a somewhat prolonged calcination.

The silica produced by the action of water on the gaseous fluoride of silicium, presents the same characters.

The shells of infusoria are also formed of amorphous silica of a density of 2·2, rising to 2·3 by calcination in a porcelain kiln.

The two modifications of silica are not merely distinguished by their density; they differ very much in the resistance which they oppose to solvents. Solutions of caustic alkalies and alkaline carbonates, as well as hydrofluoric acid, dissolve the amorphous silica with very great facility when compared with crystalline silica; this remark applies also to quartz rendered amorphous by fusion, as well as to amorphous silica obtained in the humid way.

Amorphous silica is found in nature in the form of opal and hyalite. These minerals possess a density of about 2·14 to 2·16. But these rather low numbers depend upon the fissured structure of the minerals; for when their density is determined after having reduced them to a very fine powder, it is found to be about 2·2.

Although opal is frequently found in plutonic rocks, as in basalts, it has probably not been formed there in a fused mass, but rather been produced by the prolonged action of water on the rocks. Besides, it is also found in rocks which certainly have not had an igneous origin—in the interior of fossils for instance. It is very often associated with the variety of silica whose density is 2·6. It has probably been formed by the solidification of a deposit of gelatinous silica, whilst the crystalline variety might have resulted from the slow deposit of silica in actual solution.

That which is sometimes found in the crevices of crucibles of blast furnaces, may be signalized as a particular variety of silica. M. Rose having examined some specimens, remarks that they all disengaged traces of ammonia when fused with caustic potash, whilst they disengaged nothing by simple calcination. This fact proves that this silica contains a little nitruret of silicium, and indicates that it must have been formed by the combustion of silica mixed with some of the nitruret. According to this it was very unlikely that it belonged to the crystalline variety. In fact M. Rose found its density only equal to 1·842 ; though it is true that this determination was made only upon a very small mass.

Thus, *en résumé,* we are obliged to admit the existence of two distinct modifications of silica,—one amorphous, the density of which varies from 2·2 to 2·3 ; the other crystallized, the density of which is 2·6.   This last is formed in the humid way alone, or at least in presence of water ; the first is produced either in the humid way or by fusion.   Crystallized silica is the only kind found in granite.

It is impossible not to be struck with the importance of these facts for the discussion of the theories proposed to account for the origin of granites.   Accordingly the author treats this important question in detail.

It is known that the theory of Werner on the Neptunian origin of granite was afterwards completely abandoned by geologists, and replaced by the Plutonic theory.   The latter, however, has always met with adversaries, especially among chemists, and seems to have been shaken more and more during the last few years.   MM. Fuchs, G. Bischoff, and more recently M. Delesse, particularly deserve to be mentioned as having most contributed to raise doubts as to the tenability of this theory.

Fuchs especially bases his objections upon the simultaneous presence in granite of minerals the points of fusion of which are extremely different, and upon their reciprocal penetration, which does not permit one to doubt their simultaneous formation.   He attributes also considerable weight to the absence, in granite and analogous rocks, of vitreous matter which ought to be found in the productions of an igneous fusion.

Bischoff was in like manner led to reject the hypothesis of the igneous origin of granite by the fact that in this rock felspar, an element rather fusible, is generally encrusted in mineral quartz, almost infusible ; the felspar, consequently, solidified first, which is inexplicable on the hypothesis of a crystallization produced by the cooling of a melted mass.   But he forms his opinion also on the considerations drawn from the study of all the elements of granite.

One of the elements of granite, felspar, can be artificially obtained either in the humid or dry way.   Daubrée has succeeded in producing a crystallized felspar similar to that of the trachytes by the action of water on obsidian or clay in the presence of an alkali, under the influence of a high temperature and great pressure. With respect to its production by means of ignition, it was accidentally observed in the remains of a copper furnace at Sangershausen, but we have not yet succeeded in obtaining it at will ; by the fusion of felspar or its elements and by a slow cooling, only vitreous matters have been obtained.   This formation of felspar is therefore possible, but it seems to require a combination of circumstances difficult to realize.

The numerous pseudomorphous felspars in the forms of anal-cime and Laumonite, described by Scacchi, Haidinger, Bischoff, and Rammelsberg, prove moreover that this mineral is easily formed in nature in the humid way.

Mica can also be produced in both ways. The pseudomor-phous micas, in the forms of scapolite, felspar, and Andalusite, show the possibility of its formation under the influence of water. On the other hand, its presence in the lava of Vesuvius proves that it can also be formed in the igneous way. But there seems to be an essential difference in the composition of mica, corre-sponding to these two opposite origins.

Most of the micas, and particularly that of granite, contain small quantities of water and fluorine which are dissipated by calcination, so that the crystals become opake and lose their brightness. On the contrary this does not occur with the vol-canic micas, which contain neither water nor fluorine. This observation seems to establish the fact that the mica of granites must have been formed under the influence of water, and not of heat.

But it is the examination of the quartzose elements which especially forces us to reject the hypothesis of the igneous origin of granite. It is recognized that quartz in granite generally moulds itself on crystals of felspar, and consequently has crystal-lized last of all, which fact is inexplicable on the hypothesis of a previous fusion. The attempt has been made to reply to this objection, by supposing that fused quartz might remain liquid at a temperature inferior to its point of fusion ; hence the theory of surfusion of M. Fournet. But, as M. Durocher remarked, the phænomena of surfusion are manifested only between very narrow limits of temperature, whilst it must here be admitted that the quartz had preserved its liquidity at least a thousand degrees below its point of fusion, which appears inadmissible.

The quartz of veins often contain water or other volatile liquids, hydrated oxide of iron, carbonate of iron, and other minerals decomposable by heat, which facts, as M. de Sénarmont justly remarked, evidently prove its aqueous origin. With re-spect to the quartz of granites, it is often found in the form of smoky quartz, its colour arising from a volatile or combustible sub-stance, probably carbonaceous, which is expelled by calcination. It is impossible to explain the separation, in a fusible mass, of a very basic silicate, such as mica, from free silicic acid in the form of quartz, which, at an elevated temperature, plays the part of an energetic acid. Their formation in the humid way implies, on the contrary, nothing contradictory; for we know that at a low temperature silica scarcely plays the part of an acid, carbonic acid and even water surpassing it in energy in this respect.

The remarkable purity of the quartz of granites is also in opposition to this theory, which would make of this element, in a state of surfusion, a mother-liquor from which the other crystallized minerals of granites had by degrees separated themselves.

If the external appearance of granites is not that of a fused mass which has crystallized by slow cooling, like that of devitrified glass, neither have we ever succeeded in producing, by the slow cooling of a mass of fused granite, a mass of crystalline structure. In this manner vitreous masses similar to obsidian have alone been obtained.

The author mentions on this subject an interesting experiment by his brother, M. G. Rose. When a granite rich in quartz is subjected to fusion, felspar and mica fuse and gradually dissolve a part of the quartz, but a portion of the latter remains in the form of grains or nuclei in the middle of the vitreous mass. A granite very rich in quartz, from Warmbrunn in Silesia, having been subjected to the heat of a porcelain furnace, was transformed in this manner into a mass of obsidian, still containing white nuclei of quartz. These nuclei were carefully separated from the obsidian, when they were found to be formed of amorphous silica, whose density was from 2·34 to 2·35, and which was acted on by hydrofluoric acid with the energy peculiar to this variety.

Lastly, M. Rose regards as a strong objection against the igneous origin of granite, the presence of minerals, such as Gadolinite, Allanite, &c., which at a temperature more or less elevated, suddenly present the phænomenon of incandescence, at the end of which they experience a permanent modification of their properties, and become less capable, or even incapable of being acted upon by acids. These minerals are found exclusively in granite rocks ; and their presence proves that these rocks have not been exposed to a temperature sufficient to determine their metamorphosis, which nevertheless generally requires only a dark red heat.

M. Scheerer having remarked in some of these minerals an increase of density after their ignition, endeavoured to explain their presence in granite rocks, to which he attributes an igneous origin, by supposing that during the very slow cooling of these rocks, giving rise to a contraction to which the mass did not completely yield, the elements of these minerals assumed a peculiar state of separation and tension. This explanation falls before the fact verified by M. Rose, that all these minerals do not present this increase of density ; *samarskite,* for example, has a less density after its ignition. In order to meet the objection which geologists might raise, that pressure might oppose

the metamorphosis of these minerals in granite, the author endeavoured to heat them in closed glass tubes full of air, the expansion of the air by the heat giving rise in this case to a considerable pressure. He proved that incandescence took place quite as well in this case—in fact, that it was even facilitated by pressure.

One might, it is true, suppose that these minerals, especially Gadolinite, were formed at the same time as the granite and by fusion, but that afterwards, under the prolonged influence of the atmosphere, water, an elevated temperature and perhaps other causes, they had undergone a metamorphosis accompanied by a fixation of caloric, and that thus they had passed into another, isomeric state in which they are capable of disengaging their latent heat by calcination and of presenting the phænomenon of incandescence.

Prof. Rose observes that this hypothesis contains nothing contrary to the views which he advances. Certainly no one imagines that the elements of granite have been in a complete state of aqueous solution, from which, by degrees, they have been separated by crystallization. It is possible that these elements proceed from an anterior rock which had an igneous origin, and which had assumed the crystalline state under the influence of water, heat, and pressure, as in the experiments by which M. Daubrée succeeded in obtaining several crystallized minerals. A similar hypothesis has often been published, particularly by Mr. Sterry Hunt. It has the advantage of explaining the absence of organic remains in granite, although this rock might have been formed after the appearance of organized beings.

It is clear that these observations apply, not only to granites, but to all crystalline rocks containing quartz, especially to quartzose porphyries and trachytes. The hypothesis of the igneous origin of these rocks is incompatible with the actual state of our chemical knowledge.

VII. *Description of a new Actinometer.*
By Thomas Woods, *M.D.*[*]

PROFESSOR DRAPER, of New York, published a paper in this Magazine for September 1857, in which he showed that a solution of peroxalate of iron is decomposed by light, protoxalate of iron being formed with evolution of carbonic acid—

$$\text{Fe}^2\text{O}^3, 3\text{C}^2\text{O}^3 = 2\text{FeO}, \text{C}^2\text{O}^3 + 2\text{CO}^2.$$
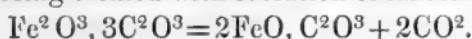
In order to find the amount of actinism which had caused the change, the quantity of protoxalate produced, or of the carbonic

acid which had been given off, was to be measured: but herein lay the difficulty; it was required to find the amount of gold precipitated from its chloride by the proto-salt in the solution of peroxalate which had been exposed to the light, or to measure by weight or volume the carbonic acid which had been evolved during the same exposure. Now, as Mr. H. C. Draper says in a paper published in the Photographic Journal for last September, "even an enthusiast would soon tire of daily following out these details." Indeed the labour would be too great, even if the results were rigidly exact. The latter gentleman, Mr. H. C. Draper, suggests the weighing of the apparatus in which the peroxalate is exposed, both before and after the exposure, taking the precaution to expel the dissolved carbonic acid by means of a stream of hydrogen, and thus to find the amount of fixed air generated by the loss of weight the apparatus sustains. This idea is ingenious; but the process would be very little if at all less troublesome than the others.

An easy and expeditious plan for the measurement of the actinic effect of light is a great desideratum. It would relieve the art of photography of half its failures, and would be of still greater advantage to its science. In order to give a helping hand towards its attainment, I have endeavoured to render the use of the peroxalate of iron as a photometric agent, both manageable and simple.

If after exposure any process is required to define the quantity of change effected by the light, especially any process involving knowledge of chemistry or nicety of manipulation, no doubt it will be neglected, except perhaps by the few "en- Fig. 1.
thusiasts," whose results would therefore be of limited value. For this reason I have endeavoured to find a method of measuring the photometric changes at once, and by the eye only, in the following manner:— having nearly filled a phial with a solution of peroxalate of iron, I passed through its cork a glass tube into the bottle, both tube and cork fitting air-tight in their places. This tube, open at both ends, dipped by one of them under the surface of the solution, so that when the bottle was exposed to light, any carbonic acid evolved should collect over the fluid, pressing it into the tube; and a scale applied to the latter would show at once the amount of action going on. A reference to fig. 1 will explain the construction of the Actinometer. A is a low-sized square phial, capable of holding about two ounces. B is the tube passing through the cork into the solution of peroxalate of iron. The carbonic acid collects in the space over
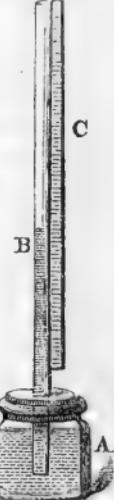
the solution, and the fluid is raised correspondingly in the tube, and read off on the scale C.

This is the principle of the actinometer; and for taking an occasional observation the above answers pretty well; but there are other circumstances to be taken into account in the construction of a more perfect instrument. For instance, if the temperature varies, the indications are interfered with, and that to an extent the greater the more sensitive the apparatus is. For it is obvious that the sensibility of the actinometer may be carried to any extent by making the tube proportionally small in the bore; but then for the same reason any change of temperature will correspondingly affect the rising of the fluid. Therefore for accurate measurements it will be necessary to have a thermometer dipping into the solution, and a preliminary experiment made in order to find to what extent the change of temperature affects the instrument. It will also be convenient to have a second tube passing through the cork, but not into the fluid, closed of course by a cork or stopcock when the instrument is in use. This tube is for the purpose of allowing the carbonic acid to escape when desirable, or for filling the vessel or emptying it when the solution is exhausted of peroxalate. The apparatus I would therefore recommend, and which I have tried (only, however, for a few days) with apparent success in determining the actinic action of light, may be seen in fig 2. It shows half the real size of the instrument I used.

Fig. 2.

A is the phial, B the tube dipping under the surface of the solution. C a thermometer, also dipping into the solution, whose graduated scale serves too as a scale for reading off the height to which the fluid ascends in the tube. D is the smaller glass tube passing through the cork, but not into the solution. When the fluid is raised nearly to the top of the tube B by the pressure of the carbonic acid, it may be drawn down again and set to any mark by opening partially the tube D until sufficient fixed air escapes to allow it to descend.

I give in the following page a representation of the heights to which the fluid rose during different parts of two days.

Height to which fluid rose in tube.	Hour of observing.	Height to which fluid rose in tube.	Hour of observing.
	h m		h m
	8 A.M. Dec. 8.		8 A.M. Dec. 9, when shaken.
			. Dec. 9.
10		7	
8		5	
7		4 15	
4 50		4	
4 15		3 45	
3 45		3 30	
3 35		3 15	
		3	
2 45		2 45	
2 35		2 30	
		2 15	
		2	
		1 45	
1 30		1 30	
1 25		1 15	
		1	
		12 45	
1		12 30	
		12 15	
12 45		12	
		11 45	
		11 30	
		11 15	
		11	
		10 45	
12 30		10 30	
		10 15	
		10	
		9 45	
		9 30	
12 20 Dec. 7.		9 A.M. Dec. 8.	

These figures give the extent of rise of the fluid in the tube on December 7 and December 8. The former commenced at 20 minutes after 12; and the fluid rose to the first mark when half-past 12, to the second mark at 45 minutes after 12, and so on. During the night it rose about one inch. December 8 was a duller day. It is marked each quarter of an hour.

This actinometer was set, as seen by the lowest mark, at 9 A.M., December 8. The fluid rose in the tube as marked at the several quarters of an hour. During the night the fluid rose from 5 to the first 8 A.M., December 9. On briskly shaking the bottle, more carbonic acid was disengaged, and the fluid rose to the highest mark.

The marks indicate the successive heights of the fluid at the different hours mentioned. The cock D was turned two or three times to allow the fluid to descend, as otherwise it would have overrun the tube.

The thermometer is not registered in these trials, as the temperature did not vary more than one or two degrees. The actinometer was at a closed window; the weather was dull and rainy, except occasionally, as at 12 o'clock December 7th, and 2 o'clock December 8th. In the sunshine, in the open air, the rise of the fluid was about three times as great as the largest space in the same period of time. The bore of the tube I used was $\frac{1}{6}$th of an inch in diameter, and the solution in the bottle exposed a surface of about three square inches. I mention these particulars, as on them depends the sensibility of the instrument. The larger the surface of fluid exposed, of course the greater will be the action of the light; and the smaller the bore of the tube the greater will be the rise for a given evolution of carbonic acid. I tried a thermometer tube of about $\frac{1}{50}$th of an inch in diameter, and the fluid rose rapidly, perhaps a couple of inches in a minute, but in jerks and irregularly; and I cannot yet say how far the bore may be diminished with utility.

When the fluid is first exposed it shows no evolution of carbonic acid, although the action of the light produces it. The gas is dissolved, to a certain extent, in the fluid, and until the latter is saturated no rise in the tube occurs. The point of saturation is reached after a greater or less time, according to the light, generally in about fifteen minutes. This is only a small inconvenience; and I got rid even of this loss of time by having a slight excess of oxalic acid in the fluid, and by adding a couple of grains of carbonate of potash, so that saturation was at once accomplished. But there is a more serious annoyance; the fluid having been saturated with the carbonic acid, gives it out again even in the dark, so that until it all nearly disappears the fluid continues to rise; the removal, therefore, of the actinometer from the light does not immediately stop the rising. This would not affect the indications if the height of the fluid in the tube was marked immediately before removing it; but there is a danger that the carbonic acid of saturation may be partially escaping into the space above the fluid, even during the continuance of an observation, so that we may have two sources of evolution, one the action of the actinism at the moment, and the other the saturated state of the fluid. For instance, if a strong light fell on the instrument, the gas would be generated quickly; if the light then diminished, the carbonic acid which would then be given out, might be due both to the action of the diminished light and to the saturated state of the fluid. I cannot say

positively whether such a double action does take place; it appears probable; but it might be, that as long as the light continues to fall on the solution, the saturated state is kept up, and consequently none comes off, except what is at the moment produced. This point will require some trials to decide. The extent to which the rise occurs is, however, not great, as may be seen by referring to p. 42, and might have been due in this case to an accidental circumstance.

If I had time to make experiments with this instrument, I would not publish this account of it, as the doubts I have expressed might be solved, and more certain results might also be obtained by using tubes of various diameters, until the most proper for all purposes would be found: but I have done all I expect to do for an uncertain period; I therefore give the description of the actinometer as it is, and for what it is worth, to the scientific world, believing it to be at least the germ of a useful and interesting instrument. It may be of use to the photographer as a means of exactly measuring the time of exposure of a sensitive plate. The period of time, as reckoned by seconds, will not always give the same amount of actinic force, as the light may vary considerably between two experiments, and yet not affect the eye. If, however, a good picture be obtained during the rise of, say two degrees on the actinometer, the same amount of actinism must always be present during the same rise, be the time of rising longer or shorter. To science also it ought to be a valuable help if its indications are sufficiently reliable.

I should have mentioned that the strength of the solution of peroxalate of iron I employed was 35 grains to the ounce of water; but I believe this strength might be advantageously increased. It will also be necessary, for comparative experiments, to have a cover for the actinometer, in which an aperture is cut of a certain size, say one or two square inches, in order that a known extent of surface may be always acted on. The tube will also require to be covered in delicate experiments, as the light acts on the fluid in it, as well as on that in the bottle.

I must advise those persons who adopt the rough and ready method of manipulating, that in making this apparatus, simple as it may appear, there is great caution to be observed in causing the stopper of the bottle to be air-tight, and also the apertures through which the tubes and thermometer pass. When the fluid rises in the tube, a great pressure is sustained by the interior of the phial; and if this be not thoroughly provided against, the air will find some small hole, too minute for observation, by which, very gradually, almost insensibly, to escape; and so the results would be vitiated.

Parsonstown, Dec. 10, 1859.

P.S. Since the above was written I have made some experiments with the actinometer, and it has answered my expectations. I believe it is a reliable register of the amount of action of light. The most important precaution to be taken in its use is to guard against change of temperature, or to have a previous knowledge of the extent to which the change will affect the instrument. Increase of temperature seems to act on it in three different ways. It expands the liquid and confined air, causing the liquid to rise in proportion to the relative bulk of the vessel and tube, as in a thermometer. It expels the carbonic acid from the saturated solution in addition to that produced by the light : the amount of carbonic acid a fluid can dissolve depends on the temperature ; the higher the latter, the less gas the fluid can contain ; so that if during a lengthened observation the temperature increases, carbonic acid is expelled independent of actinic action. And thirdly, the higher the stationary temperature is, the greater seems to be the power of the light. In this, as in all chemical processes, heat increases the action. For instance, during a day's exposure, when the thermometer was at 36° F., the fluid of the actinometer rose about 3 inches, whereas in the same period of time, when the thermometer was at 60° F., it rose about three times as high. Whether this increase was altogether due to the light acting more energetically on the warmer fluid, or partly to the higher temperature expelling some carbonic acid, I cannot at present decide.

I have used a solution of peroxalate of iron, 35 grains to the ounce ; and as it may facilitate matters for others who may wish to try the instrument, I will describe in detail how I obtained the solution.

I dissolved in 6 oz. of water 1043 grs. of protosulphate of iron ; I added 180 grs. of sulphuric acid of 1·84 spec. grav., and boiled ; while boiling I threw in 140 grs. of nitric acid of 1·42 spec. grav. This caused an effervescence of nitrous acid, for which the operator should be prepared by having the vessel of sufficient capacity and under a flue. The protosalt was thus converted into the persalt. Red prussiate of potash should now produce no blue colour. I then precipitated the peroxide of iron with ammonia and washed with warm water. I had thus 300 grs. of anhydrous peroxide of iron ; I diffused this in 20 oz. of water, and added 720 grs. of crystallized oxalic acid—an excess of about 10 grs. of acid. This dissolved the iron and gave me a solution of 712 grs. of the peroxalate—about 35 grs. to the ounce. The bottle of the actinometer holds about 2 ounces, and I used this quantity of fluid without renewing it for some weeks. How long it may retain its power I cannot say.

Parsonstown, December 23, 1859.

VIII. *On the possibility of finding a Root, real or imaginary, of every Equation.* *By* Professor Challis*.

AS the proof of the proposition that every equation has a root is at this time attracting the attention of mathematicians, I am desirous of adding a few considerations to those contained in two articles on this subject, which I communicated to the Numbers of the Philosophical Magazine for February and April 1859.

1. The proposition belongs to a branch of pure calculation, which is antecedent to, and altogether independent of, the relations of space; and consequently the proof of it does not *necessarily* involve the consideration of either lines or angles. The use that has been made of geometry of two and of three dimensions in proofs that have been recently proposed, can only be regarded as an auxiliary means of exhibiting the variations of the value of a function corresponding to variations of its variables, and not by any means as essential to the demonstration of the proposition.

2. In all the proofs that I am acquainted with, as in that which I have given in the articles above referred to, the unknown quantity x is assumed to be represented by a function of the form $z + y \sqrt{-1}$, z and y being real quantities, positive or negative. The reasons for this assumption, which are not usually much dwelt upon, appear to be such as follow. An equation may always be supposed to be formed according to the conditions of a proposed question; and its object is to discover some unknown quantity which is the answer to the question. In the formation of the equation, the unknown quantity is brought into relation with certain known quantities by operations conducted in accordance with the given conditions, and by algebraic rules. The operations are necessarily algebraic, because the relative magnitudes of the given quantities and the quantity sought for are unknown; and it is the essential principle of abstract algebra to furnish rules and symbols of operation which are proper for calculating independently of the knowledge of relative magnitudes. On account of this necessary generality in algebraic operations, the final equation involves conditions not contained in the proposed question, and its dimensions are determined accordingly. When the equation is formed, the unknown quantity becomes an *algebraic* function of the given quantities, the exact form of which in certain cases may be actually found. In all other cases such a function can be obtained in the form of a series, by the following, or some equivalent method.

Let the equation be of five dimensions; and if any terms be

wanting, let it be transformed, by adding a given quantity to its roots, into an equation in which no coefficient is zero, as

$$x^5 + px^4 + qx^3 + rx^2 + sx + t = 0.$$

Then supposing that $x = At + Bt^2 + Ct^3 + $ &c., it may be readily shown by the method of the reversion of series, that

$$x = -\frac{t}{s} - \frac{rt^2}{s^3} - (2r^2 - q)\frac{t^3}{s^4} - \text{&c.}$$

If it be supposed that $x = a + bs + cs^2 + ds^3 + $ &c., the same method gives, b, c, d, &c. by means of simple equations as functions of a; but a itself is given by the equation

$$a^5 + pa^4 + qa^3 + ra^2 + t = 0.$$

From this equation a value of a may be obtained by the process just indicated, and thus x will be expressed in a series proceeding according to the powers of s. Similar reasoning applies to the other coefficients.

These different series for x might be proper for finding *real* roots of equations; but as they are not necessarily convergent, they do not prove that a root can always be found. They show, however, that x is an *algebraic* function of the coefficients; and as every algebraic function reduced to numbers is of the form $z + y \sqrt{-1}$, it may consequently be assumed that x is of that form.

3. Hence $z + y \sqrt{-1}$ may be substituted for x in the given equation $f(x) = 0$; and as after this substitution it does not cease to be an equation, we shall have

$$f(z + y \sqrt{-1}) = 0,$$

or

$$P + Q \sqrt{-1} = 0,$$

P and Q being real functions of z and y. I am aware that mathematicians who have given especial attention to this question, have not thought themselves at liberty, after substituting $z + y \sqrt{-1}$ for x, to equate the result to zero, but have endeavoured to prove by independent considerations that there are values of z and y which will make P and Q vanish simultaneously. I confess that I am unable to see the necessity for this course of reasoning, which has the disadvantage of requiring a peculiar and complicated analysis, of the validity of which it is difficult to judge. It being once admitted, on the grounds above indicated, that the unknown quantity of an equation may have the form $z + y \sqrt{-1}$, it must surely be also admitted that this expression may be put in the place of x without destroying the equation. According to the view that I take, the resulting

equation $P + Q \sqrt{-1} = 0$, being equivalent to $P = 0$ and $Q = 0$, *proves* that there are values of z and y which make P and Q vanish simultaneously; and it only remains to show that they can be found, which may be simply done as follows. From the equations $P = 0$ and $Q = 0$, one of the unknown quantities z and y may be eliminated by a direct process; and as it has a real value, the resulting equation has a real root. Consequently, the original equation being given with numerical coefficients, this root and the corresponding value of the other unknown quantity may be found by approximate methods. Thus the possibility of finding a root, real or imaginary, of any proposed numerical equation is demonstrated.

Cambridge Observatory,
   December 21, 1859.

IX. *Chemical Notices from Foreign Journals.* By E. Atkinson, *Ph.D., F.C.S., Teacher of Physical Science in Cheltenham College.*

[Continued from vol. xviii. p. 459.]

QUERCITRINE, the colouring matter first discovered by Chevreul in quercitron bark, has since been found to be contained in a great number of plants of different orders; among others Rochleder has found it in the horse-chestnut. Rigaud found that it belonged to the class of glucosides, and was capable of being decomposed into grape-sugar and quercetine.

$$C^{58} H^{30} O^{34} + HO = C^{12} H^{15} O^{15} + C^{46} H^{16} O^{20}.$$
Quercitrine.   Glucose.   Quercetine.
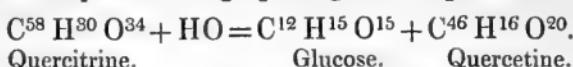
Hlasiwetz* has now found that quercetine itself can be resolved into two substances, one of which is a saccharoid matter, and the other is a weak acid. Quercetine is boiled with a concentrated solution of potash for some time, the mass then diluted with water, and filtered off from a flocculent substance which forms, the nature of which from its small quantity could not be determined. The solution is then evaporated to dryness, extracted with alcohol, the alcoholic solution distilled off, and the residue dissolved in water. To the solution, sugar of lead is added, which causes a copious precipitate.

The solution filtered off from the lead precipitate, and evaporated, deposited after some time crystals which, by analysis and from their properties, were identified with phloroglucine, the saccharoid substance formed as a product of decomposition of phloretine†.

The precipitate produced as above by acetate of lead is mixed

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* Liebig's *Annalen*, October 1859.   † Phil. Mag. vol, xi. p. 203.

with water and decomposed by sulphuretted hydrogen, and the liquid filtered from the sulphide of lead formed. This filtrate yielded, on careful evaporation, a substance crystallizing in fine silky needles, which possessed a feebly acid reaction, and in appearance and properties greatly resembled gallic acid. This substance Hlasiwetz has named *quercetic acid.* Its most characteristic property is its relation to oxygen. When a drop of alkali is added to a very dilute solution of the acid, it immediately turns yellow, and when agitated in the air gradually becomes of a brilliant red. The formula of the acid was found to be $C^{34} H^{12} O^{16}$.

From its general resemblance in appearance and properties it appears to stand closest to ellagic acid, with which it is homologous.

$$\begin{array}{ll} C^{34} H^{12} O^{16} & \cdot \quad \cdot \quad \text{Quercetic acid} \\ \underline{C^{28} H^{6} O^{16}} & \cdot \quad \cdot \quad \text{Ellagic acid} \\ C^{6} H^{6} \end{array}$$

If this be the formula of quercetic acid, that of quercetine must be altered. Hlasiwetz considers that it has the formula $C^{46} H^{16} O^{20}$, and expresses its decomposition by the following equation:—

$$C^{46} H^{16} O^{20} + 2 HO = C^{12} H^{6} O^{6} + C^{34} H^{12} O^{16}.$$
Quercetine.   Phloroglucine. Quercetic acid.
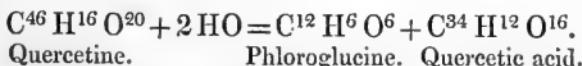
Hence the original formula for quercitrine must be altered. Quercitrine contains the elements of sugar, of phloroglucine, and of quercetic acid.

$$\begin{array}{l} C^{70} H^{36} O^{40} = \left. \begin{array}{l} 2(C^{12} H^{12} O^{12}) \\ C^{12} H^{6} O^{6} \\ C^{34} H^{12} O^{16} \end{array} \right\} - 6 HO. \end{array}$$
Quercitrine.
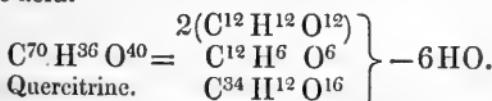
This formula would require that quercitrine should yield in its decomposition 46·3 per cent. sugar. Rigaud found 44·9 per cent. Other experiments, however, by Hlasiwetz and by Rochleder, with specimens of various preparation, yielded quantities of sugar which correspond to 1 and to 3 equivalents of sugar. It seems therefore probable that different kinds of quercitrine may exist containing different proportions of sugar, analogous consequently to the natural oils and fats with their varying quantities of fatty acids.

Rochleder * analysed a specimen of fraxetine crystallized several times from alcohol, and obtained numbers which he expresses by the relation $C^{42} H^{23} O^{27}$. But the numbers found by Rochleder agree better, as Wurtz suggests †, with the formula

$C^{42} H^{22} O^{26}$. Treated with dilute sulphuric acid, fraxine is resolved into grape-sugar and fraxetine, $C^{30} H^{12} O^{16}$. Adopting Wurtz's formula for fraxine, its decomposition may be thus expressed:—

$$C^{42} H^{22} O^{26} + 2HO = C^{12} H^{12} O^{12} + C^{30} H^{12} O^{16}.$$
Fraxine. Glucose. Fraxetine.
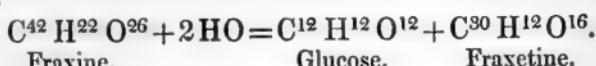
Fraxetine has an astringent taste. It is difficultly soluble in cold and in hot water: its aqueous solution has a feebly acid reaction. In its properties and formula it appears allied to esculetine, $C^{36} H^{12} O^{16}$, and quercetic acid, $C^{34} H^{12} O^{16}$.

Phloroglucine, as Wurtz points out, has the composition of phenylglycerine, the triatomic alcohol corresponding to phenylic alcohol, just as glycerine corresponds to propylic alcohol.

$C^6 H^8 O^2$ Propylic alcohol $C^{12} H^6 O^2$ Phenylic alcohol.
$C^6 H^8 O^4$ Propylic glycol $C^{12} H^6 O^4$ Phenylglycol (pyrocatechine)
$C^6 H^8 O^6$ Glycerine $C^{12} H^6 O^6$ Phenylglycerine
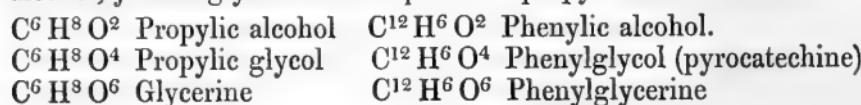
Hlasiwetz * has published the result of some researches by himself and Von Gilm on chinovine, a bitter principle, supposed to be an alkaloid, extracted from *China nova.* These investigations have shown that it belongs to the class of glucosides. Chinovine was dissolved in alcohol, and the solution saturated with hydrochloric acid gas. The liquid soon became heated, and deposited a crystalline powder which was purified by recrystallization from alcohol, in which it is difficultly soluble. *Chinovic acid,* as Hlasiwetz names this substance, forms when pure a brilliant white, crystalline powder; it is distinguished by its insolubility, its best solvent being boiling alcohol. Its solutions are very bitter. The alkaline chinovates are formed on the addition of the alkalies to a solution of the acid, as voluminous gelatinous precipitates. The salts of the alkaline earths are similar. Chinovic acid has the formula $C^{48} H^{38} O^8$, and is bibasic; it is a weak but very permanent acid. It is not attacked by hydrochloric or by boiling nitric acid. Sulphuric acid dissolves it, and deposits it unchanged on the addition of water. When distilled, odoriferous vapours are evolved, which condense to a thick, amber-yellow, resinous liquid. In its properties chinovic acid agrees most closely with Hofmann's insolinic acid, $C^{18} H^8 O^8$, and moreover, from its formula, $C^{48} H^{38} O^8$, it is homologous with it.

The acid alcoholic solution from which chinovic acid has been filtered, contains a saccharoid substance, which has the formula $C^{12} H^{12} O^{10}$. It appears to be identical with mannitane † a sub-

stance formerly regarded as anhydrous mannite. The resolution of chinovic acid might therefore be thus expressed:—

$$C^{60} H^{48} O^{16} + 2 HO = C^{48} H^{38} O^8 + C^{12} H^{12} O^{10}.$$

Chinovine.   Chinovic acid.   Mannitane.
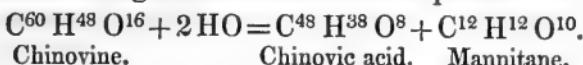
Athamantine, a crystalline substance found by Winckler in the seeds of *Athamanta oreoselinum,* is decomposed by hydrochloric acid into valerianic acid and oreoselone. Winckler and Schnedermann assigned to it the formula $C^{24} H^{15} O^7$:

$$C^{24} H^{15} O^7 = C^{10} H^{10} O^4 + C^{14} H^5 O^3.$$

Athamantine.   Valerianic acid.   Oreoselone.
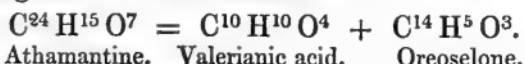
Gerhardt doubled its formula, and therewith the formula of oreoselone. Geyger has recently made some analyses which lead to the same formula; and the analysis of the nitro-compound, $C^{48} H^{27} (NO^4)^3 O^{14}$, an amorphous substance prepared by adding athamantine to cold fuming nitric acid, confirmed Gerhardt's view. By the action of chlorine on athamantine, a yellow resinous substance is formed, which has the formula $C^{48} H^{29} ClO^{14}$.

In an investigation of Kino, Eissfeldt was led to the conclusion that all plants whose aqueous extracts give a green colour with solutions of ferrous salts, yield *pyrocatechine* when submitted to dry distillation, and that all plants which give a blue or bluish-black precipitate with ferrous salts yield *pyrogallic* acid by that treatment. Eissfeldt also established the formula of pyrocatechine, which differs from that of pyrogallic acid by containing less oxygen:—

$$C^{12} H^6 O^4 \qquad\qquad C^{12} H^6 O^6.$$

Pyrocatechine.   Pyrogallic acid.

Uloth[*] has investigated the plant of the bilberry, and several allied plants which are distinguished by containing a large quantity of a substance which turns iron solutions green, and has confirmed the truth of Eissfeldt's generalization. All the plants which Uloth examined belong to the natural order of the Ericineæ, and were all found to contain, besides pyrocatechine, a crystallizable indifferent substance which he calls *Ericinone.* It was obtained as follows:—After the pyrocatechine had been precipitated from the aqueous extract of the plant by sugar of lead, the filtrate was saturated with sulphuretted hydrogen, the sulphide of lead filtered off, and the filtrate evaporated to dryness. On subjecting the mass to dry distillation, the ericinone sublimed over and condensed to white silky needles, which under the microscope are seen to consist of quadratic prisms.

It is a neutral substance, but its aqueous solution gradually decomposes, assuming an acid reaction. Even the crystals decom-

* Liebig's *Annalen,* August 1859.

pose when exposed to the air. It melts at 167°, and is thus distinguished from pyrocatechine. In its chemical relations it is indifferent; it forms no combinations with metallic oxides, in which it again differs from pyrocatechine. It reduces the oxides of the noble metals with great facility, and is completely decomposed by the alkalies. It is oxidized by nitric acid to oxalic acid, and by the action of chlorine it is converted into chloranile, $C^{12} Cl^4 O^4$. The analytical data lead to the formula $C^{24} H^{12} O^9$. It differs from pyrocatechine by containing more oxygen:
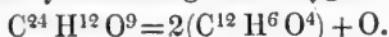
$$C^{24} H^{12} O^9 = 2(C^{12} H^6 O^4) + O.$$

X. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

May 26, 1859.—Sir Benjamin C. Brodie, Bart., Pres., in the Chair.

THE following communications were read :—

"On the Intimate Structure, and the Distribution of the Blood-vessels of the Human Lung." By A. T. H. Waters, Esq.

"On certain Sensory Organs in Insects, hitherto undescribed." By J. Braxton Hicks, M.D. Lond., F.L.S. &c.

"On Lesions of the Nervous System producing Diabetes." By Frederick W. Pavy, M.D. Lond. &c.

The author commenced his paper by stating, that all the experiments he had performed since his communication on the "Alleged Sugar-forming Function of the Liver" had been placed in the possession of the Royal Society, had confirmed the conclusions he had there arrived at. As far as his knowledge extended, it might be said that in the healthy liver during life there is a substance which he had spoken of under the term of hepatine, and which possesses the chemical property of being most rapidly transformed into sugar when in contact with nitrogenized animal materials. In the liver after death this transformation takes place, but in the liver during life there seems a force or a condition capable of overcoming the chemical tendency to a saccharine metamorphosis.

Experiments are mentioned to show that when the medulla oblongata is destroyed, and the circulation is maintained by the performance of artificial respiration, the sugar formed in the liver as a *post-mortem* occurrence is distributed through the system, and occasions the secretion of urine possessing a strongly saccharine character.

Although the destruction of the medulla oblongata leads to this effect, yet division of the spinal cord, which has been practised as high as between the second and third cervical vertebræ, has not been attended with a similar result. The brain (cerebrum) has also been separated from the medulla oblongata by section through the crura cerebri, and from the results of the experiments in which this operation has been

performed, Dr. Pavy believed that the functions of the brain may be completely destroyed, without placing the liver in the condition noticeable after actual death, or after lesion of the medulla oblongata. On account of the accidental disturbances,—such as implication of the medulla oblongata, possibly by concussion, obstruction of the respiration, and the effects of the great loss of blood sometimes attending division of the crura cerebri,—the interpretation of the result is rendered a little difficult. In an experiment, which proved most conclusive, performed to corroborate the author's previous observations whilst his communication was being written, there were none of these disturbing circumstances. In a healthy dog, during a period of digestion, the crura cerebri were completely divided. The animal was thereby thrown into a state of unconsciousness, but breathed efficiently of its own accord. The urine in an hour and a quarter's time was found perfectly free from sugar.

After poisoning by strychnine, the effect is the same as after destruction of the medulla oblongata. The circulation being maintained by artificial respiration, the urine becomes strongly saccharine.

Looking to these facts, and to the effect of Bernard's puncture of the fourth ventricle in producing diabetes, the author is led to regard the medulla oblongata as a centre, either directly presiding over the functional activity of the liver, or indirectly affecting it by altering through the medium of another or other organs the condition of the blood going to it; and he has endeavoured to establish upon positive grounds the channel by which the propagation of the nervous influence may take place. It was this line of research that conducted to the discovery of the strongly diabetic effect produced by dividing certain parts of the sympathetic.

The medulla oblongata being thus presumed to form a centre giving to the liver a force which prevents the saccharine metamorphosis of its hepatine, experiment had already shown that it cannot be through the spinal cord or the pneumogastrics separately, that the transmission of nervous influence takes place. But an experiment was performed to determine the effect of dividing both the spinal cord and the two pneumogastrics together. The cord was crushed between the third and fourth cervical vertebræ, and about half an inch of each pneumogastric was cut away from the centre of the neck. Artificial respiration was performed to keep up the circulation. The urine remained entirely free from sugar, and the liver was found in an exsaccharine state at the moment of discontinuing the respiration, and became strongly saccharine afterwards.

On next dividing all the nerves in the neck, an operation effected by performing decapitation, the result that followed after three quarters of an hour's artificial respiration was strongly saccharine urine. After this experiment, and that of division of the spinal cord and pneumogastrics, reason was afforded for looking to the sympathetic; and from the experiments that have been made and are described, the following conclusions have been arrived at. The animal selected for observation has been the dog, subsisting upon an animal diet, and operated upon at a period of full digestion.

" Division, on both sides of the neck, of the ascending branches of the superior thoracic ganglion which run up towards the canal formed by the foramina in the transverse processes of the vertebræ, for the vertebral artery, occasions an intensely marked diabetes. The urine has been found most strongly saccharine within even half an hour after the operation. The diabetic condition is only of a temporary character, passing off by the next day, and fatal pleurisy is always induced.

" Division of the ascending branches of the superior thoracic ganglion on one side of the neck only, has occasioned merely the presence of a trace of sugar in the urine in an hour and a half's time. The same operation then performed on the other side has produced in half an hour's time an intensely saccharine urine.

" *Carefully* ligaturing the two vertebral and the two carotid arteries does not lead to saccharine urine ; but when the carotids have been tied, and the tissue in connexion with the vertebrals before their entrance into the canals is a little roughly treated, without however dividing the larger sympathetic filament ascending from the superior thoracic ganglion, the urine is rendered rapidly and strongly saccharine.

" Division of the sympathetic filaments that have entered the canals does not alone produce diabetes ; but if the contents of these canals be divided, and the carotid arteries at the same time ligatured, saccharine urine is the result.

" This result is produced when the contents of the osseous canals are divided as high as the second cervical vertebra. It has also arisen after destroying the structures in the neighbourhood of the vertebral foramen on the posterior surface of the transverse process of the atlas, but has not yet been noticed after a similar operation on the anterior surface of the process.

" Dividing the contents of the canals and the tissue in immediate contact with the carotid vessels has not produced diabetes ; but when the carotids have been afterwards tied, strongly saccharine urine has resulted.

" Of all the operations on the sympathetic of the dog that have yet been performed, removal of the superior cervical ganglion the most rapidly and strongly produces diabetes. After the removal of one ganglion, the urine has been found intensely saccharine in an hour's time, and the saccharine character has remained during the following day, but has disappeared by the next. Subsequent removal of the other ganglion a few days later has been followed in half an hour's time with a strongly marked diabetic effect, which, however, has been again only of a temporary nature.

" Division of the sympathetic in the chest has been several times succeeded by saccharine urine. In one case after division on one side only, the urine was intensely saccharine in half an hour's time. On the other hand, many experiments have been made where both sides have been operated on, and only a merely traceable, or in a few instances, even no effect, has been noticeable.

" In the rabbit, removal of the superior cervical ganglia, when

the animal is in a strong and healthy state, is followed by diabetes; but the effect is not so rapidly produced as in the dog. It has been noticed at the end of four hours after the operation, and has been observed to exist until the following day.

"Excision of the superior cervical ganglia in the rabbit with a division of the pneumogastrics above their gangliform enlargement close to their exit from the skull, has been attended with the production of saccharine urine in a shorter space of time than when the ganglia alone have been removed, notwithstanding that division of the pneumogastrics in the situation referred to, has not been seen by itself to cause any positive effect."

Such is a simple statement of the principal conclusions derivable from the author's experiments, which are given in detail in his communication. As to the interpretation of the results that have been obtained, this he leaves for further investigation, in which he is now engaged, if possible, to disclose. The experiments on the sympathetic were commenced under the notion that it might form the medium of transmission of nervous force from the medulla oblongata to the liver. From this supposition certain facts have been discovered which are left for the present, without discussing whether the notion that led to them is right or wrong.

"On the Electrical Condition of the Egg of the Common Fowl." By John Davy, M.D., F.R.SS. L. & E. &c.

The structure of the egg suggested to the author the idea of its exerting electrical action. This was confirmed on trial. Using a delicate galvanometer and a suitable apparatus, on plunging one wire into the white, and the other, insulated, except at the point of contact, into the yolk, the needle was deflected to the extent of $5°$; and on changing the wires, the course of the needle was reversed. When the white and yolk were taken out of the shell, the yolk immersed in the white, the effects on trial were similar; but not so when the two were well-mixed; then no distinct effect was perceptible.

Indications also of chemical action were obtained on substituting for the galvanometer a mixture consisting of water, a little gelatinous starch, and a small quantity of iodide of potassium, especially when rendered very sensitive of change by the addition of a few drops of muriatic acid. In the instance of newly-laid eggs, the iodine liberated appeared at the pole connected with the white; on the contrary, in that of eggs which had been kept some time, it appeared at the pole connected with the yolk, answering in both to the copper in a single voltaic combination formed of copper and zinc.

The author, after describing the results obtained, declines speculating on them at present, merely remarking, that in the economy of the egg, and the changes to which it is subject, it can hardly be doubted that electro-chemical action must perform an important part, and that in the instance of the ovum generally, *i. e.* when composed of a white and of a yolk, or of substances in contact, of heterogeneous natures.

"On the Mode in which Sonorous Undulations are conducted from the Membrana Tympani to the Labyrinth, in the Human Ear." By Joseph Toynbee, Esq., F.R.S. &c.

The opinion usually entertained by physiologists is that two channels are requisite for the transmission of sonorous undulations from the membrana tympani to the labyrinth, viz. the air in the tympanic cavity which transmits the undulations to the membrane of the fenestra rotunda and the cochlea; and secondly, the chain of ossicles which conduct them to the vestibule.

This opinion is, however, far from being universally received; thus, one writer on the Physiology of Hearing contends that "the integrity of one fenestra may suffice for the exercise of hearing*;" another expresses his conviction that "the transmission of sound cannot take place through the ossicula†;" while Sir John Herschel, in speaking of the ossicles, says "they are so far from being essential to hearing, that when the tympanum is destroyed and the chain in consequence hangs loose, deafness does not follow‡."

The object of this paper is to decide by experiment how far the ossicles are requisite for the performance of the function of hearing.

The subject is considered under two heads, viz.—

1. Whether sonorous undulations from the external meatus can reach the labyrinth without having the ossicles for a medium.

2. Whether any peculiarity in the conformation of the chain of ossicles precludes the passage of sonorous undulations through it.

1. *Can sonorous undulations reach the labyrinth from the external meatus without the aid of the ossicles?*

This question has often been answered in the affirmative, apparently because it has been ascertained that in cases where two bones of the chain of ossicles have been removed by disease, the hearing power is but slightly diminished. In opposition to this view, it must, however, be remembered, that the absence of the stapes, or even its fixed condition (anchylosis), is always followed by total or nearly total deafness; and the following experiments, which demonstrate the great facility with which sonorous undulations pass from the air to a solid body, indicate that the stapes, even when isolated from the other bones of the chain, may still be a medium for the transmission of sound.

Experiment 1.—Both ears having been closed, a piece of wood, 5 inches long and half an inch in diameter, was held between the teeth, and a vibrating tuning-fork C′ having been brought within the eighth of an inch of its free extremity, the sound was heard distinctly, and it continued to be heard between five and six seconds.

Experiment 2.—One end of the piece of wood used in the previous experiment being pressed against the tragus of the outer ear, so as to close the external meatus without compressing the air con-

* Mr. Wharton Jones, Cyclopædia of Surgery, Art. "Diseases of the Ear," p. 23.
† Lancet, 1843, p. 380.
‡ Encyclopædia Metropolitana, Art. "Sound," p. 810.

tained within it, a vibrating tuning-fork C′ placed within a quarter of an inch of its free extremity, was heard very distinctly at first, and it did not cease to be heard for fifteen seconds.

Experiment 3.—Three portions of wood, of the same length and thickness as that used in the previous experiments, were glued together so as to form a triangle somewhat of the shape of the stapes; the base of this triangle being placed against the outer surface of the tragus, as in the previous experiment, the tuning-fork C′ vibrating within a quarter of an inch from its apex was heard for twelve seconds.

Considering, as shown by the above experiments, the great facility with which sonorous undulations pass from the air to a solid body, it may, I think, be assumed that the undulations in the tympanic cavity may be conveyed to the stapes even when this bone is isolated from the rest of the chain, and conducted by it to the vestibule; and when it is also considered that the absence of all the ossicles, or even a fixed condition of the stapes, is productive of deafness, there is strong evidence in favour of the opinion that *sounds from the external meatus cannot reach the labyrinth without the medium of the ossicles.*

2. Is there any peculiarity in the conformation of the chain of ossicles which precludes the passage of sonorous undulations through it?

This question has also been answered in the affirmative, on account of the various planes existing in the chain; and secondly, on account of the joints existing between the several bones composing this chain.

The following experiments refer to the influence of the varying plane of the bones forming the chain, and of its articulations, on the progress of sonorous undulations through it:—

I. *Experiments illustrative of the influence of the variety of planes in the chain.*

Experiment 1.—Three pieces of wood, each 5 inches in length and half an inch thick, were glued together thus ⟋⟍ , so as to represent the planes in which the malleus, incus, and stapes are arranged in the chain of ossicles, while three similar portions were glued end to end so as to form a straight rod. A watch was placed in contact with one end of the straight rod, while the other was pressed gently against the tragus so as to shut the external meatus. The result was that the watch was heard nearly as distinctly as when in contact with the ear. When a similar experiment was performed with the angular portion of wood representing the chain of bones, the watch was also heard, but less distinctly than through the straight portion.

Experiment 2.—A tuning-fork C′, being made to vibrate, was placed in contact with one extremity of the angular piece of wood, the other being placed against the tragus of the ear; and as soon as the sound ceased to be heard, the straight portion was substituted, when the tuning-fork was again heard, and it continued to be heard for about three seconds.

Experiment 3.—A vibrating tuning-fork C′ was placed at one extremity of the angular piece of wood, the other extremity being held between the teeth; the fork was at first heard very distinctly, and when its sound could no longer be distinguished, the straight piece was substituted, and it was again heard for the space of two seconds.

Experiment 4.—Instead of the horizontal portion of wood representing the stapes, three portions of the same size were made into a triangle, and this was glued to the anterior surface of the inferior extremity of the piece representing the incus, thus . The previous experiment was then repeated with the substitution of this apparatus for the angular one, and with nearly the same result, viz. the fork was heard through the straight piece about three seconds after it had ceased to be heard by the apparatus representing the chain of bones.

Experiment 5.—A piece of very thin paper was gummed over the end of a glass tube 6 inches in diameter; to the outer surface of this paper was glued a model of the chain of ossicles similar to the one used in the previous experiment; a vibrating tuning-fork C′ being placed in the interior of the tube and within a quarter of an inch of the paper, the sound was heard through the free end of the chain placed between the teeth for ten seconds; when the sound ceased to be heard, a straight piece of wood was substituted, and the sound was not heard through it.

II. *Experiments illustrative of the influence of the articulations in the chain.*

Experiment 1.—Three pieces of wood, each about 5 inches long and half an inch in thickness, were separated from each other by pieces of india-rubber as thick as ordinary writing-paper, and they were then fastened together so as to assume the angular form possessed by the chain of ossicles. The tuning-fork C′ being placed at the free extremity of the chain, the other extremity being held between the teeth, it was found that the sound was heard as distinctly and for the same length of time, as when it passed through the chain formed of three portions glued together.

Experiment 2.—When eight layers of the india-rubber were placed between each piece of wood, there was still very little difference in the intensity of the sound from that observed when it passed through the portions glued together.

Experiment 3.—One, two, or three fingers having been placed between the first and second pieces of wood, and eight layers of india-rubber between the second and third, a very slight diminution in the intensity and duration of the sound was observed as compared with its passage through similar pieces when glued together.

Experiment 4.—The back of the hand was placed in contact with the teeth, and the end of the vibrating fork C′ was pressed against the palm; the sound was heard very distinctly for several seconds; and when it ceased to be heard, a piece of solid wood 3 inches

long was substituted, through which the sound of the fork was again heard faintly for four seconds.

The inference from the two series of experiments above detailed is, that neither the variation of the plane existing in the chain of ossicles, nor the presence of the articulations, is sufficient to prevent the progress of sonorous undulations through this chain to the vestibule.

The experiments and observations detailed above lead to the following conclusions:—

1. That the commonly received opinion in favour of the sonorous undulations passing to the vestibule through the chain of ossicles is correct.

2. That the stapes, when disconnected from the incus, can still conduct sonorous undulations to the vestibule from the air.

3. So far as our present experience extends, it appears that in the human ear sound always travels to the labyrinth through two media, viz. through the air in the tympanic cavity to the cochlea, and through one or more of the ossicles to the vestibule.

"On the Electrical Discharge *in vacuo* with an extended Series of the Voltaic Battery." By John P. Gassiot, Esq., V.P.R.S.

In a recent communication, since ordered for publication in the Philosophical Transactions, I described some experiments on the electrical discharge in a vacuum obtained by the absorption of carbonic acid with caustic potassa, and I showed that, when the discharge from an induction coil was passed through such a vacuum, the stratifications became altered in character and appearance as the potassa was more or less heated. I have also in a former paper (Phil. Trans. 1858, p. 1) shown that the stratified discharge can be obtained from the electrical machine.

A description of an extended series of a water-battery was communicated by me as far back as December 1843 (Phil. Trans. 1844, p. 39). This battery consists of 3520 insulated cells: some years had elapsed since it was last charged, and I found the zincs were very much oxidated; on again charging it with rain-water, I ascertained that there was sufficient tension to give a constant succession of minute sparks between two copper discs attached to the terminals of the battery, and placed about $\frac{1}{5}$th of an inch apart. On attaching the terminals of the battery to the wires in a carbonic acid vacuum-tube inserted about 2 inches apart, I obtained a stratified discharge similar to that from an induction coil.

The experiment was repeated with 400 series of Grove's nitric acid battery. In this case distinct sparks between two copper discs were obtained, and the luminous layers were shown in a peculiar and striking manner, thus proving that the induction coil is not necessary for the production of the striæ, as in most of the experiments the only interruption of the battery circuit was through the vacuum-tube.

I had another tube prepared, substituting for metallic points balls of gas-carbon. At first the stratified discharge was obtained as before,

while little or no chemical action took place in the battery ; on heating the potassa, the character of the stratifications gradually changed, and suddenly a remarkably brilliant white discharge, *also stratified*, was observed ; intense chemical action was at the same time perceptibly taking place in the battery, and on breaking the circuit, the usual vivid electrical flame-discharge was developed at the point of disruption.

The continuation of these experiments will necessarily occupy much time, involving, as they do, the charging of so extended a series of the nitric acid battery, and with the requisite care necessary for the proper insulation of each cell. Other phenomena were observed which require further verification ; but I hope that after the recess the result which I hope to obtain may be of sufficient interest to form the subject of a future communication.

"Note on the Transmission of Radiant Heat through Gaseous Bodies." By John Tyndall, Ph.D., F.R.S. &c.

Before the Royal Society terminates its present session, I am anxious to state the nature and some of the results of an investigation in which I am now engaged.

With the exception of the celebrated memoir of M. Pouillet on Solar Radiation through the atmosphere, nothing, so far as I am aware, has been published on the transmission of radiant heat through gaseous bodies. We know nothing of the effect even of air upon heat radiated from terrestrial sources.

The law of inverse squares has been proved by Melloni to be true for radiant heat passing through air, whence that eminent experimenter inferred that the absorption of such heat by the atmosphere, in a distance of 18 or 20 feet, is totally inappreciable. With regard to the action of other gases upon heat, we are not, so far as I am aware, possessed of a single experiment.

Wishing to add to our knowledge in this important particular, I had a tube constructed, 4 feet long and 3 inches in diameter, and by means of brass terminations and suitable washers, I closed perfectly the ends of the tube by polished plates of rock-salt. Near to one of its extremities, a T-piece is attached to the tube, one of whose branches can be screwed to the plate of an air-pump, so as to permit the tube to be exhausted ; while the gas to be operated on is admitted through the other branch of the T-piece. Such a tube can be made the channel of calorific rays of every quality, as the rock-salt transmits all such rays with the same facility.

I first permitted the obscure heat emanating from a source placed at one end of the tube, to pass through the latter, and fall upon a thermo-electric pile placed at its other end. The tube contained ordinary air. When the needle of a galvanometer connected with the pile had come to rest, the tube was exhausted, but no change in the position of the needle was observed. A similar negative result was obtained when hydrogen gas and a vacuum were compared.

Here I saw, however, that when a copious radiation was employed, and the needle pointed to the high degrees of the galvanometer, to cause it to move through a sensible space, a comparatively large

diminution of the current would be necessary ; far larger, indeed, than the absorption of the air, if any, could produce : while if I used a feeble source, and permitted the needle to point to the lower degrees of the galvanometer, the total quantity of heat in action was so small, that the fraction of it absorbed, if any, might well be insensible.

My object then was to use powerful currents, and still keep the needle in a sensitive position ; this was effected in the following manner :—The galvanometer made use of possessed two wires coiled side by side round the needle ; and the two extremities of each wire were connected with a separate thermo-electric pile, in such a manner that the currents excited by heat falling upon the faces of the two piles passed in opposite directions round the galvanometer. A source of heat of considerable intensity was permitted to send its rays through the tube to the pile at its opposite extremity ; the deflection of the needle was very energetic. The second pile was now caused to approach the source of heat until its current exactly neutralized that of the other pile, and the needle descended to zero.

Here then we had two powerful forces in perfect equilibrium ; and inasmuch as the quantity of heat in action was very considerable, the absorption of a small fraction of it might be expected to produce a sensible effect upon the galvanometer-needle in its present position. When the tube was exhausted, the balance between the equal forces was destroyed, and the current from the pile placed at the end of the tube predominated. Hence the removal of the air had permitted a greater amount of heat to pass. On readmitting the air, the needle again descended to zero, indicating that a portion of the radiant heat was intercepted. Very large effects were thus obtained.

I have applied the same mode of experiment to several gases and vapours, and have, in all cases, obtained abundant proof of calorific absorption. Gases vary considerably in their absorptive power—probably as much as liquids and solids. Some of them allow the heat to pass through them with comparative facility, while other gases bear the same relation to the latter that alum does to other diathermanous bodies.

Different gases are thus shown to intercept radiant heat in different degrees. I have made other experiments, which prove that the selfsame gas exercises a different action upon different qualities of radiant heat. The investigation of the subject referred to in this Note is now in progress, and I hope at some future day to lay a full description of it before the Royal Society.

"Photochemical Researches."—Part IV. By Robert W. Bunsen, For. Memb. R.S., and Henry Enfield Roscoe, Ph.D., Professor of Chemistry in Owens College, Manchester.

In the three communications[*] which they have already made to the Royal Society upon the subject of photochemistry, the authors showed that they have constructed a most delicate and trustworthy instrument by which to measure the chemical action of light, and by help of which they have been able to investigate the laws regulating this action.

[*] Phil. Trans. 1857, pp. 355, 381 and 601.

In the present memoir, the authors proceed, in the first place, to establish a general and absolute standard of comparison for the chemical action of light; and in the second place, to consider the quantitative relations of the chemical action effected by direct and diffuse sunlight. They would endeavour, in this part of their research, to lay the foundation of a new and important branch of meteorological science, by investigating the laws which regulate the distribution, on the earth's surface, of the chemical activity emanating from the sun.

The subject-matter of the present communication is divided under five heads :—

1. The comparative and absolute measurement of the chemical rays.

2. Chemical action of diffuse daylight.

3. Chemical action of direct sunlight.

4. Photochemical action of the sun, compared with that of a terrestrial source of light.

5. Chemical action of the constituent parts of solar light.

The first essential for the measurement of photochemical actions, is the possession of a source of constant light. This the authors secured with a greater amount of accuracy than by the method described in their former communications, by employing a flame of pure carbonic oxide gas, burning from a platinum jet of 7 millims. in diameter, and issuing at a given rate, and under a pressure very slightly different from that of the atmosphere. The action which such a standard flame produces in a given time on the sensitive mixture of chlorine and hydrogen, placed at a given distance, is taken as the arbitrary unit of photochemical illumination. This action is, however, not that which is directly observed on the scale of the instrument. The true action is only obtained by taking account of the absorption and extinction which the light undergoes in passing through the various glass-, water-, and mica-screens placed between the flame and the sensitive gas. These reductions can be made by help of the determinations given in Part III. of these Researches, as well as by experiments detailed in the present Part. When these sources of error are eliminated, it is possible, by means of this standard flame, to reduce the indications of different instruments to the same unit of luminous intensity, and thus to render them comparable. For this purpose, the authors define the photometric unit for the chemically active rays, as the amount of action produced in one minute, by a standard flame placed at a distance of one metre from the normal mixture of chlorine and hydrogen; and they determine experimentally for each instrument the number of such units which correspond to one division on the scale of the instrument. By multiplying the observed number of divisions by the number of photometric units equal to one division, the observations are reduced to a comparable standard. It is proposed to call this unit a *chemical unit of light,* and ten thousand of them *one chemical degree of light.*

According to this standard of measurement, the chemical illu-

mination of a surface, that is, the amount of chemically active light which falls perpendicularly on the plane surface, can be obtained. It has thus been found that the distance to which two flames of coal-gas and carbonic oxide, each fed with gas at the rate of 4·105 cubic cent. per second, must be removed from a plane surface, in order to effect upon it an amount of chemical action represented by one degree of light, was, in the case of the coal-gas flame, 0·929 metre, in that of carbonic oxide 0·561 metre. The chemical illuminating power, or chemical intensity, of various sources of light, measured by the chemical action effected by these sources at equal distances and in equal times, can also be expressed in terms of this unit of light; and these chemical intensities may be compared with the visible light-giving intensities. In like manner, the authors define chemical brightness as the amount of light, measured photochemically, which falls perpendicularly from a luminous surface upon a physical point, divided by the apparent magnitude of the surface; and this chemical brightness of circles of zenith-sky of different sizes has been determined. Experiment shows that the chemical brightness of various sized portions of zenith-sky, not exceeding 0·00009 of the total heavens, is the same; or, that the chemical action effected is directly proportional to the apparent magnitude of the illuminating surface of zenith-sky.

It is, however, important to express the photochemical actions not only according to an arbitrary standard, but in absolute measure, in units of time and space. This has been done by determining the absolute volume of hydrochloric acid formed by the action of a given source of light during a given space of time. For this purpose, we require to know—

v=the volume of hydrochloric acid formed by the unit of light.
h=the thickness of sensitive gas through which the light passed.
q=the surface-area of the insulated gas.
a=the coefficient of extinction of the chlorine and hydrogen for the light employed.
l=the number of observed units of light in the time t.

When these values are known, the volume of hydrochloric acid which would be formed in the time t, by the rays falling perpendicularly on the unit of surface, if the light had been completely extinguished by passing through an infinitely extended atmosphere of dry chlorine and hydrogen, is found from the expression

$$V = \frac{v}{q} \cdot \frac{l}{1 - 10^{-ah}}.$$

In this way the chemical illumination of any surface may be expressed by the height of the column of hydrochloric acid which the light falling upon that surface would produce, if it passed through an unlimited atmosphere of chlorine and hydrogen. This height, measured in metres, the authors propose to call a *Light-metre*. The chemical action of the solar rays can be expressed in light-metres; and the mean daily, or annual height thus obtained, dependent on latitude and longitude, regulates the chemical climate

of a place, and points the way to relations for the chemical actions of the solar rays, which in the thermic actions are already represented by isothermals, isotherals, &c.

In order to determine the chemical action exerted by the whole diffuse daylight upon a given point on the earth's surface, the authors were obliged to have recourse to an indirect method of experimenting, owing to the impossibility of measuring the whole action directly, by means of the sensitive mixture of chlorine and hydrogen. For the purpose of obtaining the wished-for result, the chemical action proceeding from a portion of sky at the zenith, of known magnitude, was determined in absolute measure, and then, by means of a photometer, whose peculiar construction can only be understood by a long description, the relation between the *visible* illuminating power of the same portion of zenith sky and that of the total heavens was determined. As, in the case of lights from the same source but of different degrees of intensity, the *chemical* actions are proportional to the *visible* illuminating effects, it was only necessary, in order to obtain the chemical action produced by the total diffuse light, to multiply the chemical action of the zenith portion of sky by the number representing the relation between the visible illumination of the total sky and that of the same zenith portion.

The laws according to which the chemical rays are dispersed by the atmosphere can only be ascertained from experiments made when the sky is perfectly cloudless. In the determinations made with this specially-arranged photometer, care was therefore taken that the slightest trace of cloud or mist was absent, and the relation between the visible illuminating effect of a portion of sky at the zenith and that of the whole visible heavens, was determined for every half-hour from sunrise to sunset; the observations being made at the summit of a hill near Heidelberg, from which the horizon was perfectly free.

The amount of chemical illumination which a point on the earth's surface receives from the whole heavens, depends on the height of the sun above the horizon and on the transparency of the atmosphere. If the atmospheric transparency undergoes much change when the sky is cloudless, a long series of experiments must be made before the true relations of atmospheric extinction of the chemical rays can be arrived at. The authors believe, however, founding their opinion on the statement of Seidel in his classical research on the luminosity of the fixed stars, that the alterations in the air's transparency with a cloudless sky are very slight; and they therefore think themselves justified in considering the chemical illumination of the earth's surface, on cloudless days, to be represented simply as a function of the sun's zenith distance. Although, from the comparatively small number of experiments which have been made, owing to the difficulty of securing perfectly cloudless weather, the constants contained in the formulæ cannot lay claim to any very great degree of accuracy, the authors believe that the numbers obtained are sufficient to enable them to determine the relation accord-

ing to which the chemical energy proceeding from the sun is diffused over the earth when the sky is unclouded.

From a series of observations made on June 6, 1858, the relation between the amount of light *optically* measured falling from the whole sky, and the amount (taken as unity) which, at the same time, falls from a portion of zenith sky equal to $\frac{1}{1000}$th of the whole visible heavens, has been calculated for every degree of sun's zenith distance from 20° to 90°; the results being tabulated, and also represented graphically. These numbers, multiplied by the *chemical* light proceeding from the same portion of zenith sky for the same zenith distances, must give the chemical action effected by the whole diffuse daylight. The amount of chemical light which falls from the zenith portion of sky is, however, the chemical brightness of that portion of sky. This chemical brightness has been determined, by the chlorine and hydrogen photometer, on various days, and at different hours, when the sky was perfectly cloudless. A table contains the chemical action, expressed in degrees of light, which is effected on the earth's surface by a portion of zenith sky equal in area to $\frac{1}{1000}$th of the whole visible heavens, under the corresponding sun's zenith distances from 20° to 90°. A curve representing the relation between the action and the height of the sun, shows that although the single observations were made on different years and at different times of the year and day, they all agree closely amongst themselves, and hence another proof is gained of the slight effect which variation in the air's transparency produces ; and it is seen that the total chemical action effected by the diffuse light of day may be represented as a function of the sun's zenith distance.

The numbers thus obtained have only to be multiplied by the corresponding numbers of the former table, in order to give the chemical action effected by the total diffuse light of day for zenith distances from 20° to 90°. A table and graphic representation of these numbers is given. Knowing the relation between the sun's altitude and the chemical action, the chemical illumination effected each minute at any given locality at a given time may be calculated ; this calculation has been made for a number of places for each hour on the vernal equinox, tables and curves representing the alteration of luminous intensity with the height of the sun at these places being given.

From these data it is possible also to calculate the action produced by the whole diffuse light, not only for each minute, but during any given space of time. For the following places the amount of chemical illumination expressed in degrees of light which falls from sunrise to sunset on the vernal equinox, is—

Melville Island	10590
Reykiavik	15020
St. Petersburg	16410
Manchester	18220
Heidelberg	19100
Naples	20550
Cairo	21670

Experiment has shown that clouds exert the most powerful influence in reflecting the chemical rays; when the sky is partially covered by light white clouds, the chemical illumination is more than four times as intense as when the sky is clear. Dark clouds and mists, on the other hand, absorb almost all the chemically active rays.

The chemical action of the direct sunlight was determined by allowing a known fractional portion of the solar rays to fall perpendicularly on the insolation vessel of the chemical photometer. The solar rays reflected from the mirror of a Silbermann's heliostat were passed through a fine opening of known area into the dark room, and a large number of reductions and corrections had to be made in order to obtain, from the direct observations, the action, expressed in degrees of light, which the sun shining directly upon the apparatus would have produced if no disturbing influences had interfered. This action of direct sunshine was determined on three different cloudless days for various altitudes of the sun. As the sun approached the zenith the observed action rapidly increased; thus at 7^{h} $9'$ A.M., on September 15, 1858, when the sun's zenith distance was $76°$ $30'$, the reduced action amounted to $5·5$ degrees of light, whilst at 9^{h} $14'$ A.M. on the same day, the zenith distance being $58°$ $11'$, the action reached $67·6$. This increase in the sun's illuminating power is owing to the diminution in length of the column of air through which the rays pass. If we suppose the atmosphere to be throughout of the density corresponding to a pressure $0·76$ and a temperature $0°$, and consider it as a horizontal layer, and if A represent the action effected before entrance into the atmosphere, the action, when the ray has passed through a thickness of atmosphere $=l$, is represented by

$$W_0 = A10^{-\alpha l},$$

where $\dfrac{1}{\alpha}$ signifies the depth of atmosphere through which the ray has to pass to be reduced to $\frac{1}{10}$th of its original intensity, and where l is dependent on the atmosphere's perpendicular height $=h$, and the sun's zenith distance ϕ. The numerical values of $A\alpha$ and l may be calculated from the direct observations, and hence the action W_1 effected at any other zenith distance ϕ_1, and under a pressure P_1, is found from the equation

$$W_1 = A10^{\frac{-\alpha h P_1}{\cos \phi_1 P_0}},$$

where P_0 represents the atmospheric pressure under which A and α are calculated. A comparison between the actions W_1 thus obtained, and those, W_0, found by experiment, shows as close an agreement as could be expected where the observational errors are necessarily so large.

From these experiments it is seen, that if the sun's rays were not weakened by passage through the atmosphere, they would produce an illumination represented by 318 degrees of light; or they would effect a combination in one minute on a surface on which they fell

perpendicularly, of a column of hydrochloric acid 35·3 metres in height, assuming that the rays are extinguished by passing through an infinitely extended atmosphere of chlorine and hydrogen. By help of the above formula, it is also found that the sun's rays, after they have passed in a perpendicular direction through the atmosphere to the sea's level, under a mean pressure of 0·76 metre, only effect an action of 14·4 light-metres, or that under these conditions nearly two-thirds of their chemical activity have been lost by extinction and dispersion in the atmosphere. The total chemical action emanating from the sun during each minute is therefore represented by a column of hydrochloric acid 35 metres in height, and having an area equal to the surface of a sphere whose diameter is the mean distance of the earth to the sun. Or the light which the sun radiates into space during each minute of time represents a chemical energy, by means of which more than 25 billions of cubic miles of chlorine and hydrogen may be combined to form hydrochloric acid. In like manner the amounts of chemical action have been calculated, which the sun's rays, undiminished by atmospheric extinction, produce at the surface of the chief planets. The first column of numbers gives the mean distances of the planets from the sun, the second contains the chemical action expressed in light-metres.

Mercury	0·387	235·4 light-metres.
Venus	0·723	67·5 ,,
Earth	1·000	35·3 ,,
Mars	1·524	15·2 ,,
Jupiter	5·203	1·3 ,,
Saturn	9·539	0·4 ,,
Uranus	19·183	0·1 ,,
Neptune	30·040	0·04 ,,

By aid of the formula already given, the authors have been enabled to calculate the chemical action effected each minute by the direct sunlight, not only at different points on the earth's surface, but at various heights above the sea's level. Both these series of relations are tabulated, and graphically represented. On comparing the numbers and curves giving the action of the total diffuse light with those of the direct solar light, the singular fact becomes apparent, that from the North Pole to latitudes below that of Petersburg, the chemical action proceeding from the diffuse light is, throughout the day on the vernal equinox, greater than that effected by the direct sunlight; and that in lower latitudes, down to the Equator, the same phenomenon is observed, if not for the whole, still for a portion of the day. It is further seen, that for all places, and on every day when the sun rises to a certain height above the horizon, there is a moment at which the chemical action of the diffused light is exactly equal to that of the direct sunlight. The times at which these phases of equal chemical illumination occur can be calculated; they can also be actually determined, by allowing the direct sunlight alone, and the whole diffuse daylight alone, to fall at the same time upon two pieces of the same sensitized photographic paper; the period at which both papers become equally blackened, gives the

time of the phase of equal chemical intensity. Experiment proved not only that these points of equality which the theory requires actually occur, but also that the agreement between the calculated and observed times of occurrence of the phases is very close, giving proof that the data upon which the theory is founded are substantially correct.

The formula, by help of which the chemical action of the direct sunlight effected at any place during any given time can be calculated, is next developed, and the direct solar action at the following places calculated for the vernal equinox from sunrise to sunset. Column I. gives the action of the direct sunlight during the whole day, expressed in degrees of light; Column II. the action for the same time effected by both direct and diffuse solar light; and Column III. the same action expressed in light-metres:—

	I.	II.	III.
Melville Island	1196	11790	1306 metres.
Reykiavik	5964	20980	2324 ,,
St. Petersburg	8927	25340	2806 ,,
Manchester..........	14520	32740	3625 ,,
Heidelberg..........	18240	37340	4136 ,,
Naples	26640	47190	5226 ,,
Cairo	36440	58110	6437 ,,

The authors next proceed to examine the chemical brightness of the sun compared with a terrestrial source of light. For this purpose the intensely bright light produced by a wire of magnesium burning in the air was employed. Experiment showed that the chemical intensity of the sunlight, undiminished by atmospheric extinction, is 128 times greater than that from a surface of incandescent magnesium of like apparent magnitude; or that burning magnesium effects the same chemical illumination as the sun when 9° 53' above the horizon, supposing of course that both luminous sources present to the illuminated surface the same apparent magnitude. A totally different relation was found to exist between the visible illuminating power, *i. e.* the effect produced on the eye, of the two sources in question. Thus, when the sun's zenith distance was 67° 22', the chemical brightness of that source was 36·6 times, but the visible brightness 525 times as large as that of the terrestrial source of light.

In the last section of this communication the chemical action of the constituent parts of the solar spectrum is investigated. The sun's rays were reflected from a Silbermann's heliostat, and after passing through a narrow slit, they were decomposed by two quartz prisms. The spectrum thus produced was allowed to fall upon a white screen covered with a solution of quinine, and any desired portion of the rays could be measured by a finely-divided scale, and the position noted by observation of the distances from the fixed lines. For the purpose of identifying the fixed lines in the lavender rays, the authors were, by the kindness of Mr. Stokes, allowed the use of an unpublished map of the most refrangible portion of the spectrum, prepared by that gentleman. As the various components of white

light are unequally absorbed by the atmosphere, it was obviously necessary to conduct all the measurements so quickly after one another, that no appreciable difference in the thickness of the column of air passed through should occur.

This has been accomplished, and a series of exact measurements of the chemical actions of the spectrum for one particular zenith-distance of the sun obtained. The action on the sensitive gas shows the existence of several maxima of chemical intensity in the spectrum. Between the lines G in the indigo and H in the violet the greatest action was observed, whilst another maximum was found to lie near the line I in the ultra-violet rays. Towards the red or least refrangible end of the spectrum, the action became imperceptible about the line D in the orange, but at the other end of the spectrum the action was found to extend as far as Stokes's line U, or to a distance from the line H greater than the total length of the ordinary visible spectrum. Tables and curves representing the action are given.

"On the Action of Acids on Glycol." (Second Notice). By Dr. Maxwell Simpson,

Since my last communication (Phil. Mag. Dec. 1859) [*], I have discovered a more convenient process for the preparation of chloracetine of glycol. I have ascertained that the monoacetate of glycol is as readily converted into this substance by the action of hydrochloric acid, as a mixture of acetic acid and glycol. As the monoacetate is easily obtained, and for this purpose need not be quite pure, it is possible by this method to prepare the body in question on a large scale and with great facility. It is simply necessary to conduct a stream of dry hydrochloric acid gas into the monoacetate, maintained at the temperature of 100° C., till the quantity of oil precipitated on the addition of water ceases to increase. The whole is then well washed with water, dried by means of chloride of calcium, and distilled. Almost the entire quantity passes over between 144° and 146° C. A portion of liquid prepared in this manner gave the following numbers on analysis, which leave no doubt as to its identity :—

	Theory.	Experiment.
C_8....	39·18	39·01
H_7....	5·71	5·83
O_4....	26·14	..
Cl....	28·97	..
	100·00	

The reaction which gives birth to this body may be thus explained :—

$$\left.\begin{array}{l} C_4\,H_4 \\ C_4\,H_3\,O_2 \\ H \end{array}\right\} O_4 + HCl = \left.\begin{array}{l} C_4\,H_4 \\ C_4\,H_3\,O_2 \\ Cl \end{array}\right\} O_2 + 2HO.$$
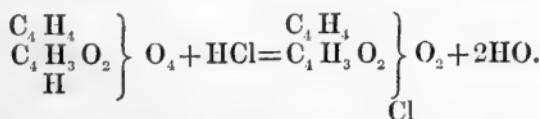
I have made a determination of the vapour-density of chloracetine, and obtained results confirmatory of the formula I have given for this body : experimental vapour-density 4·369, calculated 4·231 for 4 volumes. I have also ascertained that oxide of ethylene is formed,

and not glycol, when this substance is acted upon by a solution of potash. The following equation will explain the reaction :—

$$\left.\begin{array}{c} C_4H_4 \\ C_4H_3O_2 \\ Cl \end{array}\right\} O_2 + 2\left.\begin{array}{c} K \\ H \end{array}\right\} O_2 = \left.\begin{array}{c} C_4H_3O_2 \\ K \end{array}\right\} O_2 + KCl + C_4H_4O_2 + 2HO.$$
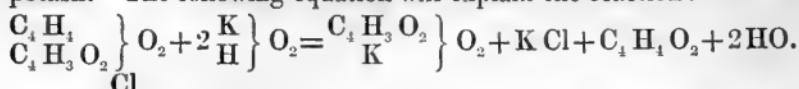
Action of Chloracetine of Glycol on Butyrate of Silver.—Formation of Butyroacetate of Glycol.

Equivalent quantities of chloracetine and butyrate of silver were exposed in a balloon with a long neck to a temperature ranging between 100° and 200° C., till all the silver salt had been converted into chloride. The product was then digested with ether, filtered, and the filtered liquor submitted to distillation. As soon as all the ether had been driven off, the thermometer rose rapidly to 180°, and between that temperature and 215° almost the entire quantity passed over. This was fractioned, and the portion distilling between 208° and 215° was set apart for analysis. The numbers obtained lead to

the formula $\left.\begin{array}{c} C_4H_4 \\ C_4H_3O_2 \\ C_8H_7O_2 \end{array}\right\} O_4$, as will be seen from the following percentage Table :—

	Theory.	Experiment.	
		I.	II.
C_{16}	55·17	54·31	55·58
H_{14}	8·04	8·20	7·97
O_8	36·79	..	..
	100·00		

I also made a determination of the acids by heating a weighed quantity of the ether with hydrate of baryta in the usual manner. The quantity of sulphate of baryta obtained indicated 2·2 equivalents of acid for one equivalent of the substance analysed. The excess of acid was probably owing to the presence in the ether of a trace of free butyric acid. The following equation will explain the reaction which causes the formation of this compound :—

$$\left.\begin{array}{c} C_4H_4 \\ C_4H_3O_2 \\ Cl \end{array}\right\} O_2 + \left.\begin{array}{c} C_8H_7O_2 \\ Ag \end{array}\right\} O_2 = \left.\begin{array}{c} C_4H_4 \\ C_4H_3O_2 \\ C_8H_7O_2 \end{array}\right\} O_4 + AgCl.$$
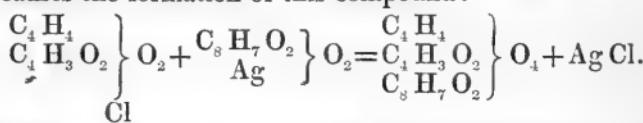
In many reactions chlorine replaces, and is replaced by, $H+O_2$; in this it is replaced by the group $C_8H_7O_2$ (equivalent to one atom of hydrogen) $+O_2$.

This ether, which I may call butyroacetate of glycol, has a bitter pungent taste. It is insoluble in water, but soluble in alcohol. It is specifically heavier than water. It is a very stable body,—solution of potash, even when boiling, effecting its decomposition with difficulty.

I have no doubt that many analogous compounds may be prepared in the manner I have just described.

Action of Chloracetine of Glycol on Ethylate of Soda.

In the hope of forming a compound intermediate between diace-

tate of glycol and diethylglycol, I resolved to try the action of chlor-acetine on ethylate of soda, thinking that probably the body in question might be generated by the following reaction:—

$$\left.\begin{array}{l}C_4 H_4 \\ C_4 H_3 O_2 \\ Cl\end{array}\right\} O_2 + \left.\begin{array}{l}C_4 H_5 \\ Na\end{array}\right\} O_2 = \left.\begin{array}{l}C_4 H_4 \\ C_4 H_3 O_2 \\ C_4 H_5\end{array}\right\} O_4 + Na\,Cl.$$
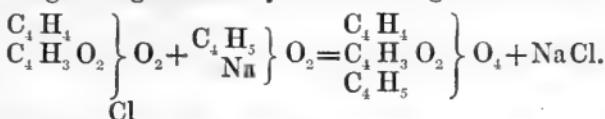
In order to settle this point, I exposed equivalent quantities of these bodies in a sealed balloon to the temperature of a water-bath for about two hours. My expectations, however, were not realized. On opening the balloon, I found that the reaction had proceeded too far, acetic ether having been formed along with the chloride of sodium.

Action of Hydrochloric and Butyric Acids on Glycol.—Formation of Chlorbutyrine of Glycol.

This compound is prepared in the same manner as its homologue, namely by transmitting a stream of dry hydrochloric acid gas through a mixture of equivalent quantities of butyric acid and glycol, maintained at the temperature of 100° C. As soon as the reaction is finished, the product is well washed with water, dried by means of chloride of calcium, and distilled. The greater part passes over between 160° and 182°. This must be rectified, and the quantity distilling between 175° and 182° collected apart. This gave, on analysis, results agreeing with the formula $\left.\begin{array}{l}C_4 H_4 \\ C_3 H_7 O_2 \\ Cl\end{array}\right\} O_2$, as will be seen from the following table:—

	I.	II.
C_{12} .... 47·84	47·76	..
H_{11} .... 7·30	7·31	..
O_4 .... 21·28	..	..
Cl .... 23·58	..	23·88

The reaction, to which the formation of this body is due, may be thus explained:—

$$\left.\begin{array}{l}C_4 H_4 \\ H_2\end{array}\right\} O_4 + \left.\begin{array}{l}C_8 H_7 O_2 \\ H\end{array}\right\} O_2 + H\,Cl = \left.\begin{array}{l}C_4 H_4 \\ C_3 H_7 O_2 \\ Cl\end{array}\right\} O_2 + 4\,HO.$$
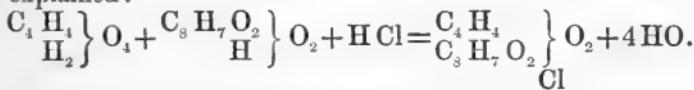
Chlorbutyrine of glycol, as I may call this compound, has a pungent and somewhat bitter taste. It boils at about 180°. Its specific gravity at zero is 1·0854. It is insoluble in water, but freely soluble in alcohol. It is decomposed with difficulty by a boiling solution of potash, but readily by solid potash,—chloride of potassium, butyrate of potash, and oxide of ethylene, being formed.

I have ascertained that acetobutyrate of glycol, the ether I have already described, can be prepared from this body as well as from chloracetine, by exposing it to the action of acetate of silver. The process is the same as that I have already given, with this difference, that the reacting bodies must not be heated above 150° C. The

ether prepared in this manner gave the following numbers on analysis :—

	Theory.	Experiment.
C_{16}	55·17	56·29
H_{14}	8·04	8·75
O_3	36·79	..

The quantity of this substance at my disposal was so small (the greater part of my product having been lost) that I could not purify it completely ; hence the experimental numbers do not exactly accord with the theoretical.

Action of Hydrochloric and Benzoic Acids on Glycol.—Formation of Chlorbenzoate of Glycol.

A mixture of equivalent quantities of glycol and benzoic acid, previously fused and powdered, was exposed to the action of dry hydrochloric acid gas for several hours, the mixture being maintained at the temperature of 100° during the action of the acid, as in the case of the former compounds. The product thus formed presented the appearance of a soft white solid, and contained a considerable quantity of uncombined benzoic acid. This was removed by agitating it with hot water, till, on cooling, it no longer became solid, but remained perfectly fluid. Finally it was dissolved in alcohol, and precipitated by water. The body thus prepared, and without being distilled, was analysed, having been previously dried *in vacuo* over sulphuric acid. Another specimen, prepared in the same manner, at a different time, was also analysed, having, however, been previously distilled. During the distillation it was observed that not a drop of fluid passed over till the mercury had risen to 254°, and between that temperature and 270° the entire liquid distilled over. What passed over between 260° and 270° was collected separately ; this was the portion analysed. The numbers obtained on analysis agree with the formula $\left. \begin{matrix} C_4\,H_4 \\ C_{11}\,H_5\,O_2 \\ Cl \end{matrix} \right\} O_2$, as the following Table shows:—

Theory.	Experiment.		Portion distilled.
	I.	II.	
C_{19} .... 58·54	59·70	..	58·69
H_9 .... 4·87	5·01	..	5·31
O_4 .... 17·35	..	..	..
Cl .... 19·24	..	17·93	..
100·00			

The portion not distilled contained doubtless a trace of free benzoic acid, which would affect the carbon and chlorine, but not the hydrogen.

Chlorbenzoate of glycol, as I shall call this compound, has a

pungent and somewhat bitter taste. It is insoluble in water, but freely soluble in alcohol and ether. Boiling solution of potash effects its decomposition with difficulty, solid potash readily, the reaction being the same as in the case of the analogous compounds.

Action of Hydriodic Acid on Glycol.—Formation of Iodide of Ethylene and Iodhydrine of Glycol.

Hydriodic acid gas is absorbed with great energy by glycol. A considerable quantity of heat is evolved during the passage of the gas, and the liquor becomes black and thick from the separation of free iodine. On removing the iodine by means of dilute potash, a mass of small white crystals is brought to light, which I at once suspected to be iodide of ethylene. To remove all doubt on this point, I submitted the crystals to analysis, having previously purified them by recrystallizing from boiling alcohol. The numbers obtained agree with the formula of iodide of ethylene :—

	Theory.	Experiment.
C_4	8·51	8·73
H_4	1·42	1·78
I_2	90·07	..
	100·00	

The reaction which causes the formation of iodide of ethylene may be thus explained :—

$$\left.\begin{array}{c} C_4\,H_4 \\ H_2 \end{array}\right\} O_4 + 2\,HI = C_4\,H_4\,I_2 + 4\,HO.$$
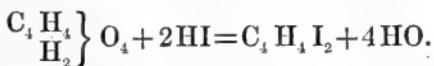
That the action of hydriodic acid on glycol should be different from that of hydrochloric acid is doubtless owing to the bond of union between hydrogen and iodine being much weaker than that between hydrogen and chlorine.

If, on the other hand, the temperature of the glycol be prevented from rising during the passage of the hydriodic acid gas, by surrounding the vessel containing it with cold water, a liquid product is obtained, which is coloured dark-brown by free iodine. This I have not as yet been able to discover any means of purifying, it being soluble in water, and decomposed by distillation. I believe, however, it is the compound corresponding to chlorhydrine of glycol $\left(\begin{array}{c} C_4\,H_4 \\ H \\ Cl \end{array}\right\} O_2\right)$ discovered by M. Wurtz. A portion of this liquid,

from which I had simply removed the free iodine, by agitation with mercury, gave, on analysis, numbers agreeing tolerably well with the formula of iodhydrine of glycol. After the analysis, however, I discovered that it contained a considerable quantity of iodide of mercury in solution. Another portion, from which I had removed the iodine by means of metallic silver, gave, on analysis, 11·1 per cent. carbon

and 3·5 hydrogen, instead of 13·9 carbon and 3·0 hydrogen. After all, an analysis is not necessary to enable us to arrive at the composition of this body. The products formed by the action of potash on it furnish us with almost as convincing a proof of its composition as any analysis could do. They are iodide of potassium and oxide of ethylene.

Iodhydrine of glycol is soluble in water and alcohol, but insoluble in ether. It has no taste at first; after a time, however, it almost burns the tongue, it is so pungent. It is decomposed by heat into iodide of ethylene, and probably glycol. It acts with great energy on the salts of silver.

Action of Hydriodic and Acetic Acids on Glycol.—Formation of Iodacetine of Glycol.

A stream of hydriodic acid gas was conducted into a mixture of equivalent quantities of glacial acetic acid and glycol, the temperature of which was prevented from rising during the action of the gas. As soon as a portion of the liquid gave a considerable quantity of an oily precipitate on the addition of water, the passage of the gas was interrupted; for the prolonged action of the gas is apt to give rise to the formation of iodide of ethylene. The liquid thus obtained was well washed with very dilute potash, dried *in vacuo*, and analysed. The numbers obtained lead to the formula $\left.\begin{array}{c} C_4 H_4 \\ C_4 H_3 O_2 \end{array}\right\} O_2$, as will be seen from the following Table:—

	Theory.	Experiment.	
		I.	II.
C_8....	22·42	21·95	22·30
H_7....	3·27	3·31	3·50
O_4....	14·96	..	..
I	59·35	..	..
	100·00		

Iodacetine has a sweetish pungent taste. It is insoluble in water, but soluble in alcohol and ether. Its specific gravity is greater than that of water. It crystallizes in tables when exposed to cold. Heated with potash, it gives iodide of potassium, acetate of potash, and oxide of ethylene. It is readily decomposed by the salts of silver.

This compound can also be prepared with great facility by exposing monoacetate of glycol to the action of hydriodic acid gas. The liquid must be kept cold during the action of the gas, which should be interrupted as soon as the addition of water to a portion of it causes an abundant oily precipitate. The whole is then washed with dilute potash, and dried *in vacuo*. A specimen prepared in this manner gave, on analysis, 22·62 per cent. carbon and 3·43 hydrogen, instead of 22·42 carbon and 3·27 hydrogen.

I hope soon to have an opportunity of studying these iodine compounds more particularly.

Action of Anhydrous Acetic Acid on Glycol.—Formation of Mono-acetate of Glycol.

A mixture of equivalent quantities of anhydrous acetic acid and glycol was heated in a sealed tube for several hours at a temperature not exceeding 170° C. On opening the tube, and submitting its contents to distillation, it was observed that the mercury remained stationary for a considerable time at about 120°, the point of ebullition of glacial acetic acid, and then rose rapidly to 180°, between which and 186° the remainder of the liquid passed over.

This was analysed, and proved to be pure monoacetate of glycol.

	Theory.	Experiment.
C_8	46·15	46·02
H_8. ...	7·69	7·80
O_6. ...	46·16	..
	100·00	

The following equation will explain the reaction which takes place between the acid and the glycol :—

$$\left.\begin{array}{c}C_4\,H_4\\H_2\end{array}\right\}O_4 + \left.\begin{array}{c}C_4\,H_3\,O_2\\C_4\,H_3\,O_2\end{array}\right\}O_2 = \left.\begin{array}{c}C_4\,H_4\\C_4\,H_3\,O_2\\H\end{array}\right\}O_4 + \left.\begin{array}{c}C_4\,H_3\,O_2\\H\end{array}\right\}O_2.$$
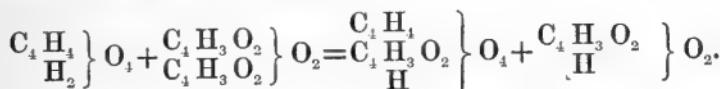
The foregoing experiments were performed in the laboratory of M. Wurtz.

GEOLOGICAL SOCIETY.

[Continued from vol. xviii. p. 479.]

November 30, 1859.—Prof. John Phillips, President, in the Chair.

The following communications were read :—

1. "On some Bronze Relics from an Auriferous Sand in Siberia." By T. W. Atkinson, Esq., F.G.S.

During the author's stay at the gold-mine on the River Shargan, in Siberia (Lat. 59° 30′ N. and Long. 96° 10′ E.) in August 1851, some fragments of worked bronze were dug up by the workmen, at a depth of 14 feet 8 inches below the surface, from a bed of sand in which gold-nuggets occur. This sand rests on the rock, and is covered by beds of gravel and sand, overlain by 2 feet of vegetable soil. The fragments appear to have belonged either to a bracelet or to some horse-trappings.

2. "On the Volcanic Country of Auckland, New Zealand." By Charles Heaphy, Esq. Communicated by the President.

The isthmus-like district of Auckland and its neighbourhood, described by Mr. Heaphy as a basin of Tertiary deposits, is bordered

by clay-slate, igneous rocks, and at one spot on the south by creta-ceous strata; and it is dotted by upwards of sixty extinct volcanos, often closely situated, and showing in nearly every instance a well-defined point of eruption, generally a cup-like crater, on a hill about 300 feet high. Interesting instances of successive volcanic eruption are numerous all over this district, 60 miles round Auckland; and there seems to have been four distinct epochs of eruption, thus classified by Mr. Heaphy :—1. The first was that which raised the trachytic mountains and the black boulder-like igneous rock. 2. Then came the eruptions in the Tertiary period, the ashes of which form beds in the Tertiary rock. 3. Then the eruptions on the upheaval of the Tertiary cliffs: these appear as cones above faults on the Tertiary beds and on the edges of cliffs. 4. Lastly the eruptions that have broken through the Tertiary beds, and the lava-streams of which follow the natural valleys of the country. The volcanic phænomena were illustrated by maps and numerous sketches by the author. Some Tertiary *Terebratulæ*, some few fossil plants, and some Cretaceous fossils (*Inoceramus* and *Belem-nitella*) accompanied this memoir.

3. "On the Geology of a part of South Australia." By T. Burr, Esq. From the Colonial Office. 1848.

The lowlands about Adelaide on the west, and along the River Murray on the east, consist of horizontal beds of limestone and cal-careo-siliceous deposits, yellowish and reddish in colour, full of marine fossils, and of the Tertiary age. Sometimes gypsum and ferruginous sand replace the limestone. These plains are arid, except where granite protrudes from the surface, presenting cavities in which rain-water collects. The author observed a similar Tertiary formation on Yorke's Peninsula, at Port Lincoln, and to the S.E. to beyond Rivoli Bay; and it probably forms vast tracts in New South Wales and Western Australia. None of these tertiary districts appear to exceed an elevation of 300 feet above the sea.

In describing two volcanos in South Australia, Mount Gambier and Mount Schauck, Mr. Burr remarked that, coming from the west or north-west, at about 20 miles from these hills a white coral-lime-stone (Bryozoan limestone) containing flint or chert, takes the place of the limestones and calcareous sandstones, with recent sand-forma-tion, previously passed over. This white limestone is remarkable for the numerous deep well-like water-holes in it, within about 12 miles of the volcanic mountains and about east or west of them.

Mount Gambier has a height of 900 feet above the sea (600 feet above the plain), and has three craters, lying nearly east and west, and occupied with lakes of fresh water. Mount Schauck, at a distance of about 9 miles, magnetic south, is circular, and has one large, and two small lateral craters.

The author next described the granite, gneiss, and slaty rocks along a section extending from the River Murray and Kangaroo Range, across Mount Barker and Mount Lofty, towards Adelaide; and noticed the mode of occurrence of the ores of copper, iron, lead, &c. in these rocks. Lastly he noticed and explained the occurrence

of calcified stems of trees, standing in the position of their growth, in the sand-dunes of the Gulf of St. Vincent, near Adelaide.

4. "On some Tertiary deposits in South Australia." By the Rev. Julian Edmund Woods.   Communicated by the President.

The author, in the first place, described the geographical features of that part of the colony of South Australia to which his observations refer.   It lies between the River Murray on the west, and the colony of Victoria on the east; and includes an area 156 miles long, N. and S., and 70 broad from E. to W.   Some trap-dykes and four volcanic hills are almost the only interruptions to the horizontality of these plains, which rise gradually from the sea, and are occupied by the Tertiary beds to be noticed; they extend into Victoria for some seventy miles, as far as Port Fairy.

In some places on the plains a white compact unfossiliferous limestone lies under the surface-soil; and is sometimes 30 feet thick. Under this is a fossiliferous limestone.   The passage between the two is gradual.   This latter rock is made up of *Bryozoa*—perfect and in fragments—with some *Pectens, Terebratulæ, Echinoderms,* &c.

Sometimes this rock appears like friable chalk, without distinct fossils.   A large natural pit, originating from the infalling of a cave, occurs near the extinct volcano Mount Gambier, and is 90 feet deep—showing a considerable thickness of this Bryozoan deposit in several beds of 14 ft., 10 ft., 12 ft. thickness.   Similar pits show the deposit in the same way at the Mosquito Plains, 70 miles north.

Regular layers of flints, usually black, rarely white, occur in these beds, from 14 to 20 feet apart.   These, with its colour, and with the superficial sand-pipes, perforating the rock to a great depth, give it a great resemblance to chalk.

The whole district is honeycombed with caves—always, however, in the higher grounds in the undulations of the plains.

One of the caves, in a ridge on the northern side of the Mosquito Plains, is 200 feet long, is divided into three great halls, and has extensive side-chambers.   The caves have a north and south direction, like that of the ridge.   The large cave has a great stalactite in it; and many bones of Marsupialia are heaped up against this on the side facing the entrance; possibly they may have been washed up against this barrier by an inflowing stream.   The dried corpse of a native lies in this cave.   It has been partially entangled in the stalactite; but this man was known to have crept into the cave when he had been wounded, some fourteen years ago.   Many of the caves have great pits for their external apertures, and contain much water.

Some shallow caves contain bones of existing Marsupialia, which have evidently been the relics of animals that fell into the grass-hidden aperture at top.

The caves appear in many cases to be connected with a subterranean system of drainage; currents and periodical oscillations being occasionally observed in the waters contained in them.   There is but little superficial drainage.   One overflowing swamp was found by

the author to send its water into an underground channel in a ridge of limestone.

Patches of shelly sand occur here and there over the 10,980 square miles of country occupied by the white limestones; but near the coast this shelly sand thickens to 200 feet.

A coarse limestone forms a ridge along the coast-line, and it contains existing species of shells. This indicates an elevation of the coast of late date, and which probably is still taking place.

XI. *Intelligence and Miscellaneous Articles.*

ON A NEW MINERAL CONTAINING NIOBIUM.
BY DR. JULIUS POTYKA.

THE author was induced by H. Rose, to undertake the investigation of a mineral received by Dr. Krantz, of Bonn, from Norway under the name of tyrite, and sent by him to Rose. The analyses of this mineral showed that it is probably a new species. Its composition is different both from that of Fergusonite (Weber), and from that of tyrite (Forbes). From these two minerals it is distinguished especially by its great amount of potash, and from tyrite also by its containing zirconia, whilst alumina has been found in tyrite. As, however, the locality where it occurs is still unknown, and its crystalline form has not yet been observed, the author has not given it a name.

The mineral received by the author forms small specimens of irregular outline about 4 lines in diameter, included in red felspar. It is not cleavable, has an uneven fracture, a black colour, and an imperfect metallic lustre; the fragments exhibit a reddish brown translucence at their edges. Its streak is reddish brown. Its hardness is equal to that of apatite.

When heated before the blowpipe with borax, it furnishes a globule which is reddish yellow while hot, yellowish when cold; in phosphorus salt it dissolves readily, forming a clear globule, which is greenish yellow while hot, greenish on cooling. When fused with carbonate of soda and nitrate of potash, it gives no reaction of manganese. The specific gravity of the coarse powder is 5·124 at 63°·68 F. If hot water be poured over the mineral, it crackles; and on boiling it afterwards, air-bubbles escape—at the same time the colour becomes pale liver-brown, but on drying it again becomes black.

When heated in a retort, the mineral decrepitates and furnishes milky aqueous drops with an odour of sulphuretted hydrogen, together with traces of sublimed sulphur; it probably contains intermixed iron pyrites.

The calcined mineral is brownish yellow; when strongly ignited in the platinum crucible, it lost in all 3·71 per cent. Its specific gravity was then 5·319 at 64°·58 F. The mineral in very fine powder is of a dingy yellow colour.

In Analysis I. the calcined mineral was decomposed by bisulphate of potash, and in Analysis II. by sulphuric acid.

A. shows the average calculated for uncalcined mineral.

B. the amount of oxygen :—

	I.	II.	A.	B.
Hyponiobic acid. . . .	45·10	45·24	43·49	8·58
Zirconia	0·83	..	0·80	0·21
Tungstic acid.	1·40	..	1·35	0·28
Oxide of tin.	0·10	..	0·09	0·02
Oxide of lead	0·43	..	0·41	0·03
Oxide of copper	0·36	..	0·35	0·07
Yttria	33·13	..	31·90	6·35
Protoxide of cerium .	3·82	..	3·68	0·53
Protoxide of iron . . .	1·17	..	1·12	0·24
Protoxide of uranium	4·28	..	4·12	0·49
Lime	2·03	..	1·95	0·55
Magnesia	trace	..	trace	
Potash.	..	7·51	7·23	1·22
Water.	..	..	3·71	3·29

The amount of oxygen in the acids to that in the bases is as 1 : 1·04, from which we may deduce the formula $3\,RO + Nb^2\,O^3$, in which the term RO includes the bases KO, YO, CeO, UO, and CaO.—*Poggendorff's Annalen,* cvii. p. 590.

THE PSEUDO-DIASCOPE. BY F. O. WARD.

By means of this instrument an aperture transmitting light is made to produce on one eye an isolated impression, while the other eye is directed to an opake body, such as the hand held before it. The image of the aperture is then found to be transposed, and its perception ceases to be assigned to the eye by which it is really seen, —the effect being that a perforation appears in the opake body, through which the light seems to shine upon the eye by which this is viewed. The principle illustrated by this instrument, according to the author's view, is the essentially goniometrical and deductive nature of the visual act, whenever the distances of bodies are perceived, and their relative positions in space assigned.—*Proc. Lit. and Phil. Soc. Manchester,* Nov. 29, 1859.

ON THE OCCURRENCE OF UREA IN THE ORGANS OF THE PLAGIOSTOMOUS FISHES. BY G. STÄDELER.

In an investigation made last year by Frerichs and Städeler, these observers found that the Plagiostomi are distinguished from all other fishes by their containing large quantities of urea in all their organs. The organs and the blood of *Scyllium canicula,* the kidneys and muscles of *Spinax acanthias,* and different organs of the Rays, con-

tained urea in abundance, whilst in bony fishes, as also in the Sturgeon and Lamprey, which make the nearest approach to the Plagiostomi, no urea could be detected. From this it appeared that the metamorphosis of materials in the Plagiostomi is quite different from that in all other fishes, and the subject was of sufficient importance to call for further investigation.

Last winter the author saw upon a table a large specimen of *Raia clavata*, which had been taken at Marseilles, and sent to Zurich during very cold weather. The author was able to procure the salt water in which the fish was boiled, and succeeded in preparing pure urea therefrom.

The gills, heart, liver, spleen, kidneys, pancreas, testes, the humours of the eye, the lenses and the muscles of a large specimen of *Raia Batis*, which the author subsequently obtained from Havre, contained very large quantities of urea, accompanied by the substances formerly mentioned. No trace of uric acid could be detected. Creatine was found not only in the muscles, but also in the heart and the branchiæ ; in the muscles it was accompanied by another, difficultly soluble body, which was precipitated in white flakes by pernitrate of mercury, and also formed a compound with silver when ammonia was carefully added. This body was therefore possibly allantoine. The quantity of scyllite which the author obtained, principally from the liver, was not sufficient for an elementary analysis.

The author obtained two Torpedos from Professor Lessona of Genoa, *T. ocellata* and *marmorata*. They were young specimens of about 3 inches in breadth and from 4 to 5 inches long, preserved in spirit. The alcohol had penetrated all the organs; and therefore, as a separate examination of these could lead to no result, the objects were pounded with powdered glass and extracted with alcohol. The alcoholic extract was treated as already described. Urea was present in abundance. Thus the occurrence of urea is proved with regard to six fishes of the order Plagiostomi (viz. *Scyllium canicula, Spinax acanthias, Raia Batis, R. clavata, Torpedo marmorata,* and *T. ocellata*). As regards the formation of urea in these animals, the author indicates that, as no trace of uric acid occurs in the Rays, it appears to be most probable that the urea is formed by the further decomposition of creatine, the latter taking up water and splitting into urea and sarcosine :—

$$\underbrace{C^8\,H^9\,N^3\,O^4}_{\text{Creatine.}} + 2HO = \underbrace{C^2\,H^4\,N^2\,O^2}_{\text{Urea.}} + \underbrace{C^6\,H^7\,NO^4}_{\text{Sarcosine.}}$$
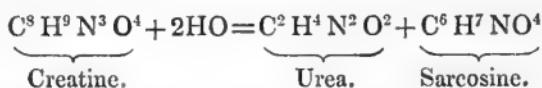
If the above-mentioned body, precipitable by mercury and silver, be actually allantoine, the urea might certainly be derived from this, by its taking up water and oxygen and becoming decomposed into carbonic acid and urea.—*Journ. für Prakt. Chemie,* lxxvi. p. 58.

XII. *On Vowel Sounds.* By M. HELMHOLTZ[*].

A MUSICAL note is produced by a periodical motion of the air repeated in the same manner at equal and sufficiently small intervals of time. The motion during each period of oscillation may be quite arbitrary, provided that the same motion which took place during the first period be repeated in like manner in all subsequent ones.

If, in each period of oscillation, the small particles of air move to and fro exactly in the same manner as the centre of gravity of a pendulum when its amplitude is very small, we hear only a simple and single note, whose pitch is determined by the number of equal periods contained in a second. In this case the velocity as well as the pressure of the air at any point of the mass of air in motion, may be represented mathematically by a simple expression of the form $A \sin (2\pi nt + c)$. In a former memoir on resultant notes (*Combinations-Töne*), I have pointed out a method by means of which simple pendulum-like oscillations of molecules of air (or, as I proposed to call them, *simple aërial waves*) may be produced. To do so I made use of tuning-forks, which, when struck, do not communicate perceptible oscillations to the mass of air in which they are held. But when they are held at the mouth of a resonant tube whose deepest note is in unison with that of the tuning-fork, this deepest note of the fork is communicated forcibly to the air. Even when the tuning-fork can give still higher notes, it may be easily so arranged that these, its higher notes, shall not be in unison with the higher notes of

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[*] Translation, from Poggendorff's *Annalen*, vol. cviii. p. 280, of a paper originally communicated by the Author to the Royal Bavarian Academy of Sciences.

the resonant tube, and thus, not being reinforced by the latter, shall remain inaudible.

But when the motion of the air during a period of oscillation does not follow the simple law of the pendulum, but any other whatever, we may in general by due attention discover various notes, even when the motion of the air is produced by a single sounding body. Now, according to the well-known theorem of Fourier, every periodical motion of the air may be expressed mathematically by a sum of terms, each of which has the form $A \sin (2\pi mt + c)$, and therefore corresponds to a simple pendulum-like oscillation of the particles of air. In this expression, A and c are dependent upon the value of m, and m assumes successively the values n, $2n$, $3n$, $4n$, &c., where n, as before, denotes the number of simple periods in a second.

Now in all cases where the nature of the motion of the sounding body can be theoretically found and mathematically represented as a sum of such sines, the ear, when due attention is paid, can really distinguish notes of n, $2n$, $3n$, &c. oscillations, although, in all cases in which such a motion of the air is not actually produced by different sounding bodies, the coexistence of a number of simple pendulum-like oscillations of the particles of air is a pure mathematical fiction.

The universality of this perception of distinct notes induced a celebrated member of this Academy, the late G. S. Ohm, to propose as a definition of a simple note that which is produced solely by a simple pendulum-like motion of the air of the form $A \sin (2\pi mt + c)$. This definition of a note given by Ohm was vehemently attacked by Seebeck, who maintained that it was too narrow, and that the sensation of a single note might also be produced by a motion of the air which differs considerably in form from that of a simple pendulum oscillation. I cannot here enter into a complete refutation of the objections raised by Seebeck, and must therefore return to the subject on another occasion. I will only remark that his objections are founded essentially on the difficulties which we often experience in distinguishing the higher notes. In fact, in all observations made by the senses two things must be kept distinct, viz. the immediate sensation or effect upon the auditory nerves, and the conception which arises therefrom by a psychical process, and which leads us to the conviction of the presence of a certain sounding body. In the immediate sensation the several simple notes may certainly be distinguished from one another when sufficient attention is given ; whilst in the mental image they become blended together into the impression produced upon the ear by the tone of the sounding body in question. The attention, indeed, generally requires artificial assistance in order to distinguish the several

elements of the compound sensation,—just as, for example, special methods of observation are requisite in order to convince ourselves that the apparent solidity of any object at which we look arises from the coalescence of different pictures presented to our two eyes.

On this account I formerly proposed to designate by the name *sound* (*Klang*) the whole compound sensation produced by the motion of the air proceeding from a single sounding body, and to limit the name *note* (*Ton*) to the simple sensation produced by a simple pendulum-like motion of the air. Accordingly the sensation of a sound is generally composed of the sensations of several simple notes. If we limit to sounds all that Seebeck said in his discussion with Ohm, and to notes the assertions of Ohm, both these distinguished acousticians are right, and the assertions of both may remain undisturbed side by side.

We will retain this designation throughout, and at the same time agree to understand by the pitch of a sound the pitch of the gravest simple note of n oscillations contained therein, *i. e.* the pitch of its *fundamental* or *primary* note; all others will be called incidental or higher notes. I call the note of $2n$ oscillations, the octave of the foregoing, the *second note*; that of $3n$ oscillations the *third note*, and so on.

The generosity of His Majesty the King of Bavaria having enabled me to procure the apparatus necessary for my research, I proposed to examine the consequences of the proposition of Ohm on the theory of tones (*Klangfarbe*). In a physical point of view, it has long been known that the different forms of the aërial waves within each single period of oscillation correspond to what our ear distinguishes as different tones or qualities of sound. But this hypothesis rested solely upon the fact that there was no other possible way of explaining such differences of tone. An experimental verification was requisite, and this is, perhaps, now supplied by my researches.

In a physiological point of view, a further consequence could be drawn from the proposition of Ohm. Since all oscillations which do not correspond to the simple motion of a pendulum produce sensations in which a certain number of simple notes are distinguishable, sounds of different qualities whose primary notes have equal pitch, must, to the ear, be rendered different by the different intensities of the harmonic incidental notes. Let us leave out of consideration the different ways in which sounds of different instruments and voices commence and cease, as also the manifold shouting, grating, jarring, irregular noises which accompany many of them, and which, properly speaking, ought not to be considered as constituting the musical part of the sounds, and let us call the part of the tone

which does not depend upon the above-named accidental circumstances, the *musical tone or quality* of the sound; the question then arises, *Do musical tones differ only in consequence of the different intensities of the incidental notes contained therein?*

But in conceiving the form of a wave composed of several simple waves, it is of importance to consider not only the amplitudes of oscillation of the latter, but also the *differences of phase* between them and the primary note. We obtain very different wave-forms when we combine the wave of a primary note and its first higher octave, according as we allow the maximum condensation of the primary to coincide with that of the octave, or with the minimum condensation of the octave, or with any other intermediate phase. The former question, therefore, becomes included in the following more special form: *Does the distinction of musical tones depend only upon the perception of higher notes of different intensities, or does the ear also distinguish differences of phase?*

This question would be most directly answered by endeavouring at once to produce sounds of different qualities by direct combinations of simple notes, such as can be obtained with tuning-forks. The several vowels of the German language appeared most appropriate as objects for imitation, for they may be produced as uniformly continuous musical sounds, and at the same time be kept nearly, though not quite, free from unmusical noises.

My apparatus consists of a series of eight tuning-forks, which correspond to B (in the deepest octave of a bass voice), and to its harmonic higher notes as far as b_2 (in the highest octave of a soprano), namely to the notes B, b, f_1, b_1, d_2, f_2, as_2, and b_2. Each tuning-fork is fastened between the ends of a horseshoe electro-magnet and joined to a properly tuned resonant tube. The mouths of the resonant tubes are provided with moveable covers, which may be removed by means of threads whose ends are fastened to a set of pianoforte keys. The tuning-forks are set in motion by means of intermittent electric currents, which are produced according to the principle of Neef's hammer, and whose number per second is equal to 112, the number of the oscillations per second of the deepest-toned fork. After overcoming several difficulties, I succeeded in so arranging the apparatus that, when put into action, the low humming of the forks could scarcely be heard so long as all the resonant tubes remained closed; but as soon as one or more of the resonant tubes were opened by means of the pianoforte keys, the respective notes became distinctly audible. The intensity of any note could be easily regulated by opening the corresponding tube more or less completely.

I combined in the first place the two deepest notes alone, to

these I then added the third, and gradually several others, and endeavoured to imitate with the voice the sounds thus produced. Thus I learnt by degrees to imitate the different sounds of the vowels more or less completely; U, O, Oe, E were pretty good and distinct; I, Ue somewhat less so, for here the whistling of the air through the mouth, to whose different characters Donder called attention, is comparatively loudest; A and Ae were still worse, because here we require the combination of a great number of notes, the intensity of each of which cannot be so completely regulated; for A, in fact, a series of higher notes, for which I had no forks, were requisite.

It may be remarked that, in general, the vowel-sounds composed by means of tuning-forks are more similar to those of the human voice when singing than when speaking. In the dry sound of our ordinary speech another kind of intonation is chosen, in which the primary note is much less prominent than the higher incidental notes and noises; by this means, in fact, the differences of tone become more evident than they do in singing, where the primary note becomes more intense, and thus hides the incidental notes more completely. The artificially produced vowels bear the strongest resemblance to those which are heard when we sing those vowels loudly into the interior of a pianoforte. The following are the particulars of my results:—

The simple primary note, compared with the compound sounds, had the tone of U. The vowel is somewhat more distinct when the primary note is weakly accompanied by the third note.

O is imitated when the primary note is powerfully accompanied by its higher octave. A very weak accompaniment of the third and fourth notes is advantageous, though not necessary.

E is especially characterized by the third note, the second being moderately strong. The fourth and fifth may also be weakly sounded.

The transition from O to E, therefore, follows from diminishing the second and increasing the third note.

Oe ensues when both these secondary notes are loud.

Ue arises when the primary note is accompanied by a third note of moderate strength.

For I, the primary note must be weakened, the second, in comparison with the primary note, must be strong, the third very weak, the fourth, which is characteristic of this vowel, must be loud, and the fifth moderately strong. The weak notes, such as the third and fifth, may be omitted without causing any essential change of quality.

For A and Ae, on the contrary, the higher incidental notes are characteristic; the second note may be quite omitted, the third may be weakly given, but the higher notes must be

made as prominent as possible, for by the method here em-
ployed the intensity of the highest is but small.   For Ae, the
fourth and fifth notes are specially important; for A, the notes
from the fifth to the seventh.   When the third note is com-
pletely omitted, A has a nasal sound.

I must, however, remark that the above-mentioned relations
between the primary and the higher notes refer only to the pitch
of my tuning-forks. The primary note B corresponds nearly to the
pitch of moderately deep male voices when speaking.   I have not
yet had time to conclude my researches on vowels at a higher
pitch, for I was not able to pursue the investigation much
further with my incomplete set of tuning-forks.   When I made
the former second note *b* my primary, I had only three appro-
priate higher notes.   With these I was able to imitate U, O, Oe,
E, Ue, and I according to the given rule; but the absence of the
higher notes rendered my imitation of A and Ae imperfect; so
that here, as at a lower pitch, the same relation of upper notes
to the primary one appeared to be essential in the imitation of
the vowel.   This higher pitch corresponds nearly to that in
which high voices generally speak.

On the other hand, I carried the investigation further by
direct observations on the human voice by means of a special
contrivance, which renders the most inexperienced capable of
distinguishing the incidental notes of every musical sound,—a
problem which formerly could be solved only by long practice
and great attention.   I made use of peculiar resonant vessels,
which were applied to the ear itself.   The best vessels of this
kind are glass globes with two openings, one of which termi-
nates in a funnel-shaped neck whose end fits into the ear.   If
one ear is provided with such a resonant globe whilst the other
is closed, most external notes are very much deadened; those,
however, which correspond to the proper note of the glass globe
(in combination with the hollow of the ear) are heard with ex-
traordinary distinctness.   The upper notes of any external sound
which correspond to the note of the glass globe, are now also
increased in intensity.   If, for example, a globe is placed to the
ear whose note is f_1, and the vowels are sung on B, whose third
note is f_1, it will be found that with U, I, Ue, A, and Ae, the
note of the globe is only feebly heard, whilst it becomes very pro-
minent with O and Oe, and extremely intense with E.   By the
help of such resonant globes a number of acoustical phænomena,
such as objective resultant notes, the incidental notes and their
beats, which were formerly difficult to investigate, are rendered
easily accessible.   The investigation of the human voice, so con-
ducted, confirmed the results which I had obtained with the
tuning-forks when B was the key-note sung upon; for keys of

higher pitch there were slight deviations. It was found that for the incidental notes of several vowels, certain parts of the musical scale are peculiarly favourable, so that these notes, falling in this part of the scale, become stronger than when they fall in other parts. Thus, for O, the upper half of the octave above the lines constitutes such a favourable part of the scale. The third and fourth higher notes, which are heard distinctly at a low pitch of the vowel, lie in this part, and are not so prominent when O is sung on a higher key. For A, the upper half of the second octave above the lines is favourable. The second, third, and fourth notes, which are weak at the low pitch of the vowel, are very prominent when A is sung between b and b_1. Moreover, I found by means of the above-described resonant globes that, especially for A, there are feeble but audible notes higher than any to which my tuning-forks reach. When the vowel A was sung on F, another globe, which was tuned to es_3, corresponding to fourteen times as many oscillations as F, resounded considerably.

With respect to differences of phase, no effect of the kind manifested itself in my experiments. I was able to control the phases of oscillation according to the optical method of Lissajou. In the first place, by reversing the electrical currents in the electro-magnet of every single tuning-fork, the oscillation of the latter can be changed by half an undulation, so that the maximum and minimum deflections change places with each other. Further, by fixing a little wax to them, the tuning-forks can be slightly untuned; their oscillations then become weaker, and thus, up to a quarter of an undulation, the phases can be the more displaced the greater the discordance of the tuning-forks. The change of phase in the weaker notes may be still more easily effected. To do so, they may either be weakened by removing the forks further from the resonant tubes, whereby the phases of the oscillations of the air are not changed, or the resonant tubes may be only partially opened; in the latter case a change of phase takes place, as I have shown in a theoretical memoir on Acoustic Oscillations, which is now being printed in Crelle's Journal (vol. lvii.). The changes of phase produced in any one of these ways, however, cause no change in the tone, provided the intensities of the notes remain the same; so that the former question may in general be answered thus: *The musical tone depends only upon the presence and intensity of the incidental notes in the sound, and not upon their differences of phase.*

I must, however, remark that there are apparent exceptions to this rule. When the notes are sufficiently strong, resultant notes may become intermixed, which, according to the differences of phase, may partly weaken and partly strengthen the primary notes, so as to give rise to differences of tone. Here, however,

amongst other experimental results, I believe I may venture to assert that the differences of sound depend only upon the differences in the intensities of the notes; but that the latter, under the above-mentioned circumstances, depend upon differences of phase.

For the present, however, I would prefer to limit the above assertion to the lower incidental notes, which, lying far apart in the scale, reach as far as the sixth and seventh. The higher incidental notes give discords and beats with each other; and when a number of such pairs of notes which give rise to beats are heard together, it is probably not indifferent, as far as perception is concerned, whether the pauses of all these beats fall together or not. The latter, however, depend upon the differences of the phase. Moreover, I hold it to be probable that all these higher dissonant incidental notes form what the ear recognizes as accompanying noises, which latter we have already excluded from our consideration of musical tones.

I have in another place been led to the hypothesis, that each nervous fibre of the auditory nerve is destined for the perception of notes of a particular pitch, and is excited when the note which strikes the ear corresponds in pitch to that of the elastic formation* in connexion with the fibre. According to this, the perception of different tones would reduce itself to the simultaneous excitation of the fibre which corresponds to the primary note, and of certain others corresponding to the incidental notes. This simple explanation could not have been given had the differences of phase of the lower incidental notes entered into consideration.

XIII. *A Theory of Molecular Forces.*
By Professor CHALLIS†.

THE general Theory of Physical Forces, the principles of which I have indicated in previous communications, must, if it have a real foundation, include a theory of molecular forces; that is, of the forces by which the constituent atoms of bodies are held in different states of aggregation—as the solid, the fluid, and the gaseous. The inquiry into the laws and modes of action of this class of forces has long engaged the attention of physicists, and has given rise to a great variety of special hypotheses, mostly of an arbitrary kind, and not referable to any general principle. The theory I am about to explain differs from all that have preceded it in this respect, that it admits of no other kind of action than the pressure of a very elastic fluid medium (the æther), and

* " Des Cortischen Organs oder Borste in den Ampullen."
† Communicated by the Author.

no law of force which is not a mathematical deduction, by means of hydrodynamical equations, from the assumed dynamical property of the medium that its pressure is proportional to its density. The history of physical science seems to show that theoretical investigation proceeds in but one course, that of deducing quantitative laws, by means of solutions of equations, from known or hypothetical principles. For example, by the solutions of the first order of differential equations, the *law* of *vis viva* is deduced from dynamical principles known by experiment, and from D'Alembert's *principle.* By the same class of equations, Kepler's laws are readily deduced from certain hypotheses respecting the force of gravity. In the latter instance, one of the hypotheses is, that gravity varies inversely as the square of the distance from the centre of emanation. As this hypothesis may also be called a quantitative law, it may, according to these views, be presumed to be itself deducible from ulterior principles by means of a higher order of equations. This is what I have attempted to do in a communication to the Philosophical Magazine for December 1859.

If this course of investigation applies to one kind of force, it is reasonable to suppose that it applies to all. It is a matter of demonstration that a theory of molecular forces cannot be constructed on the hypothesis that the forces vary according to some law of the distance from individual material particles, unless the law be such that the force changes sign with the distance, so as to become attractive after being repulsive. But if force be a virtue resident in the particle, it must *at its origin* be either attractive or repulsive, and it seems impossible to conceive how by emanation to a distance it can change its quality. This difficulty, as will be shown, is not encountered in a theory of molecular forces, which deduces their laws from the dynamical action of an elastic medium.

Again, on the same principles it is not permitted to ascribe to the ultimate atoms of matter any variable quantitative properties. Accordingly I assume in the following theory, as I have done heretofore, that, while different atoms may be of different magnitudes, their magnitudes and forms are constant, and that all have the same intrinsic inertia. The property of constancy of form might be otherwise expressed by saying that the atoms are infinitely hard. Further, I make the more particular hypothesis, that all atoms have the form of a sphere. It would be contrary to these principles to ascribe to an atom the property of elasticity, because, from what we know of this property by experience, it is quantitative, and, being most probably dependent on an *aggregation* of atoms, may admit of explanation by a complete theory of molecular forces.

To these preliminary remarks I beg to add the expression of my conviction, that theoretical physics can advance only in such a course as that above indicated, and that progress will be made in proportion as the difficulties which attend the application of partial differential equations to physical questions are overcome. I do not consider the following theory to be free from such difficulties.

1. It is an evident consequence of the hypothesis that substances consist of discrete atoms, that neighbouring atoms are mutually repellent, for they could not otherwise remain in positions of equilibrium. This action is the repulsion of *heat.* It will not be necessary to show here in what manner such repulsion results from the dynamical action of undulations of the æther, because I have discussed this question in the Mathematical Theory of Heat contained in the Philosophical Magazine for March 1859, and at present I have nothing better to offer on this part of the subject. There are, however, some mathematical considerations, relating equally to repulsive and attractive action, which may now be appropriately introduced.

In an article on Attractive Forces, contained in the Philosophical Magazine for last November, I have investigated the pressure at any point of the surface of a given atom, due to the incidence of a given series of waves, on the assumption that, for the case in which the excursions of the particles of the æther are large compared to the diameter of the atom, the velocity V along the surface of the hemisphere on which the waves are incident is W sin θ, and along the surface of the other hemisphere,

$$\mathrm{W} \sin \theta - q \cdot \frac{d\mathrm{W}}{dt} \sin \theta \cos \theta.$$

In this expression, W is put for $m \sin\left(\dfrac{2\pi b t}{\lambda} + c\right)$, the velocity of the ætherial particles; θ is the angle which the radius to the point considered makes with the radius drawn in the direction contrary to that of incidence; and q is a certain constant. These values of the velocity were deduced in the Philosophical Magazine for December, from a particular solution of the general partial differential equation to terms of the first order, of which P, or Nap. log ρ, is the principal variable, viz.

$$\frac{d^2\mathrm{P}}{dt^2} = b^2 \cdot \left(\frac{d^2\mathrm{P}}{dx^2} + \frac{d^2\mathrm{P}}{dy^2} + \frac{d^2\mathrm{P}}{dz^2}\right).$$

The following is a more general value of V satisfying the same solution:

$$\mathrm{V} = \mathrm{W} \sin \theta + \left(\mu\mathrm{W} - q\frac{d\mathrm{W}}{dt}\right)\sin\theta\cos\theta;$$

or, differently expressed,

$$V = m \sin \theta \sin\left(\frac{2\pi bt}{\lambda} + c\right) + m\nu \sin \theta \cos \theta \sin\left(\frac{2\pi bt}{\lambda} + c'\right),$$

μ and q, and by consequence ν, being in general functions of m as well as λ, and depending also on the magnitude of the atom. If the last expression be applied to the velocity along the first hemispherical surface, $\nu = 0$, the velocity impressed by the waves incident on that surface being $W \sin \theta$. For waves having large values of λ and large excursions of the particles, such as those which came under consideration in the Theory of Gravity, the factor $\mu = 0$, because, on account of the small size of the atom, there is no sensible difference between the velocities along the surfaces of the first and second hemispheres, excepting that which was shown to be proportional to $\dfrac{dW}{dt}$, and to be due to the varying momentum of the fluid which passes the plane separating the two hemispheres. On the other hand, for waves whose particles perform excursions very small compared to the diameter of an atom, q must be very small, because the fluid in contact with the second hemisphere is disturbed but to a small extent, and the varying momentum just spoken of has very little effect. In this case we have very nearly

$$V = W \sin \theta (1 + \mu \cos \theta).$$

Now it is evident that V and W must have the same sign, and consequently that $1 + \mu \cos \theta$ does not change sign. Hence the limiting value of θ is the arc whose cosine is $-\dfrac{1}{\mu}$, which, if μ be a very large positive quantity, exceeds but little $\dfrac{\pi}{2}$. Thus the conditions assumed in the mathematical theory of heat are satisfied by supposing μ to be very large and q to be very small; and the fulfilment of these conditions accounts for the great energy of calorific repulsion. For as the fluid in contact with the second hemispherical surface is nearly undisturbed, the pressure on the other is not counteracted by opposite pressure; and as the total effective pressure on the first surface varies nearly as the *square* of the radius of the atom, while the quantity of inert matter of the atom varies as the *cube* of its radius, it follows that the expression for the acceleration contains the radius of the atom in the denominator. Hence atoms of very small size, acting upon each other by the intervention of waves of which the excursions are very small, mutually repel with a very great force; and at the same time, as was shown in the Theory of Heat, the

force varies very rapidly with the distance. We have now to consider how this repulsion is controlled by attraction.

2. Conceive the atoms contained in a spherical surface of radius R to be centres of undulations propagated from them equally in all directions, and take any point at a distance D from the centre of the sphere, such that the straight lines drawn to it from the atoms are *quam proxime* parallel. Then $\frac{R}{D}$ being a very small but fixed ratio, let the number of atoms included within the spherical surface of radius R be a very large given number N. It is conceivable that this number may be so large that the resultant consecutive values of the condensation at the given distance D, which must be as often *plus* as *minus*, may be expressed by one or more circular functions, in which the values of λ are very much larger than those for the component undulations. In fact, as the components may be supposed to have values of λ very nearly consecutive, there will be epochs of coincidence, or greatest proximity, of their maximum condensations, and equidistant epochs of coincidence, or greatest proximity, of their maximum rarefactions. The fixed number N is determined by the condition, that the resultant of the different series of waves of the first order propagated from the individual atoms, becomes at the distance D a series of waves of another order, analytically expressible like the first by periodic functions. As the waves of the second order cannot, any more than those of the first, be regarded as due to a specific disturbance, but as resulting from the mutual action of the parts of the fluid, both ought, according to the hydrodynamical principles which I have adopted, to be equally expressed by periodic circular functions.

The effect above described is analogous to what takes place at the surface of water disturbed within a limited space, it being observable that, whatever be the mode of disturbance, at a short distance from it are formed and propagated concentric rings of alternate depression and elevation, which to all appearance have continuous boundaries, and are probably the resultant of subordinate series of waves, which have their origins at innumerable points at the place of disturbance.

3. For the sake of distinction, the portion of any given substance which consists of the fixed number of atoms N, will be called a *molecule*, whether the space containing them be *cubical* or *spherical*. If the molecule be of the form of a cube, the quantity represented by R must be understood to be the radius of the sphere which has the same solid content as the cube. First, let the substance be in the state of aggregation of a *solid*. Then, the mean interval between the atoms being

small), the radius R of a molecule will be small, and as $\dfrac{R}{D}$ is a fixed ratio, D will also be comparatively small.   Hence, as the condensation propagated from each atom varies inversely as the distance, it may be supposed that the resultant condensation and corresponding velocity of the ætherial particles at the distance D from the centre of the molecule, are so large that the excursions are large compared to the diameter of an atom.   Thus the dynamic effect of the new order of waves will be an *attraction* towards the centre of the molecule.   The mathematical investigation of the amount of this attraction will be the same as that I have given in the Theory of the Force of Gravity (Numbers of the Phil. Mag. for November and December 1859); and the expression for the acceleration of any atom will consequently be

$$-\frac{3\pi^2 qam^2}{8\kappa\lambda^2}$$

at a position where the maximum velocity of the waves is m.   It is here to be remarked that, as the value of λ is much smaller for this class of waves than for those which were supposed to account for the force of gravity, this molecular attraction will be much more energetic, for the same value of m, than the attraction of gravity.

It follows from this reasoning that the waves propagated from the atoms of a given molecule have no repulsive action at the distance D, their dynamic action having merged into that of the second order of waves.   The atomic repulsion due to the part of the velocity which is unaccompanied by condensation, must vanish at a much less distance than D, on account of its varying inversely as the *fourth* power of the distance.   That due to the part of the velocity accompanied by condensation vanishes more slowly, but is at its origin comparatively feeble.   In this manner the theory accounts for the small sphere of activity of the atomic repulsion.

If we consider apart the dynamic action of the same molecule at distances much greater than D, the condition that the excursions of the particles of the medium are very large compared to the diameter of an atom, must at a certain distance cease to be satisfied; the factor q will continually diminish, and the factor μ become significant, till the molecular attraction will be changed to *molecular* repulsion.   But the amount of this repulsion, which will depend on the relative magnitudes of μ and q, may be very much less than the *atomic* repulsion, and vary much less rapidly with the distance.   Also if we take a spherical space of radius R′, containing N molecules of N atoms, N being the same fixed number as before, and suppose the molecules to be of the form

of a cube in order that they may fill the space, then by the same reasoning as before, at a distance D' from the centre of the sphere such that $\frac{R'}{D'} = \frac{R}{D}$, the waves of the second order will merge into waves of the *third* order. It is to waves of this order that the force of gravity may be attributed. Also the absorption of the second order of waves into the third, puts a limit to the sphere of activity of the second order of repulsion.

As an illustration of the formation of the waves of the second order was drawn from what is observed to take place at the surface of water in consequence of its being disturbed through a limited extent, so the third order of waves are analogous to the *ocean-swell*, or series of long waves, which have been observed on shores at great distances from parts of the ocean which have been agitated by a violent storm.

I have elsewhere made the remark, that even the attraction of gravitation may, according to these views, be changed by distance into repulsion, so that neighbouring stars may be repulsive to each other, while at the same time this repulsion is counteracted by an attraction resulting in the manner above described from the composition of the waves propagated from all the other more distant stars. Thus the final waves may be said to be of the *fourth* order, and the masses of stars and planets may be regarded as molecules relatively to the material system of the universe.

4. The above considerations respecting molecular forces apply equally to a mass in a *fluid* state, the number of atoms in a given space being not so different in the fluid and solid states of the same substance as to render any difference in the reasoning necessary. But experience shows that the molecular *attraction* of a fluid mass is much less powerful than that of the same mass when solid. This difference, which theoretically corresponds to a difference in the relative magnitudes of μ and q, is chiefly exhibited in the different circumstances of the equilibrium of the atoms at the *boundary* of the mass, on which, in fact, the difference between the solid and fluid states essentially depends. If we take an atom in the interior of a uniform mass, and regard only the action of forces having very small spheres of activity, it is evident that whether the mass be solid or fluid, the repulsions to which the atom is subject will counteract each other, as will also the attractions. But the case will be different if the atom be situated at the boundary of the mass; for there, to maintain its equilibrium, the resultant of the attractions must be just equal and opposite to the resultant of the repulsions. This point I have considered at length in an article " On Capillary Attraction and the Molecular Forces of Fluids," communicated to the Philosophical Magazine for February 1836, on the suppositions

that the atoms are isolated, and that the sphere of activity of attraction is much larger than that of repulsion. These suppositions are in accordance with the views now expounded; and the explanation there given of the conditions of equilibrium of an atom at the boundary applies in the present theory. The principal hypothesis of that explanation is one first admitted by Poisson, viz. that, within a distance from the bounding surface very small compared to the radius of activity of the molecular attraction, there is a rapid increase of density from the surface towards the interior. The effect of such change of density will be to diminish very much the atomic repulsion on an atom at the surface, while the molecular attraction, on account of its far greater sphere of activity, will be unaffected by it. The change of density must be such that the atomic repulsion at the surface is reduced to an equality with the molecular attraction, the latter prevailing beyond the surface.

The conditions of the equilibrium of the atoms situated at and near the *surfaces* of bodies, bring this molecular theory into relation with *electricity.*

The difference between the circumstances of the equilibrium of the superficial atoms of solids and fluids, on which, as said above, the difference between the solid and fluid states depends, consists, according to these views, in the different amounts of the resultant molecular attractions acting in directions parallel to and very near the surface, and tending to prevent the separation of the atoms in those directions. In fluids, as experience teaches, this is a very feeble force; in solids it is overcome by *cutting,* or by *fracture,* resuming its sway in the new surfaces which these operations produce. Atomic arrangement seems to have much to do with the energy of this force.

Both solids and fluids offer great resistance to compression within a smaller space. This resistance is due to the *atomic* repulsion, and its energy depends both on the great amount of this force, and on its rapid variation with distance.

It is also a matter of experience that, when the parts of a substance (not fluid) are separated, in general they strongly resist being joined together again so as to form a single mass. This fact may be accounted for if we suppose that the molecular attraction which acts on the atoms situated at the boundary of the solid, passes through a phase of repulsion before the waves to which it is due merge themselves in those that give rise to the attraction of gravitation. But independently of such repulsion, it is evident that the gradation of density at the boundaries, being due to the cause assigned above, must be destroyed before separate portions of the same substance can be perfectly united. In cases in which the union is opposed by no energetic *molecular*

repulsion, extending, as above stated, to small distances from the surface, it is conceivable that mere mechanical compression of fragments together, by acting in aid of the molecular attraction, may suffice entirely to get rid of the gradation of density, and thus to effect a perfect union. The very important and instruct-ive experiments of Professors Tyndall and Huxley, detailed in the Transactions of the Royal Society (vol. cxlvii. pp. 329–331), are actual instances of the production of this effect by crushing together fragments of ice.

As a theory of forces, of the nature of that which I am advo-cating, can be expected to be established only by the number and variety of the explanations of physical phænomena which it gives, I take this opportunity of remarking that the foregoing molecular theory, taken in conjunction with the experiments just referred to, seems to afford a simple explanation of some of the phænomena of *glaciers*. Both from the experiments and from the theory, it may be inferred that the mutual pressures of the parts of a glacier are continually tending to obliterate fragmen-tary composition and make it a continuous whole; and as, ac-cording to the theory, the interior of a continuous solid mass is not different from that of a fluid mass, the glacier has a tendency to *flow*. The strength of its rigid casing, on account of the feeble molecular attraction of ice, not sufficing to keep the parts in the same relative positions, it flows as a *stream*, as was experi-mentally proved by Professor Forbes. The rigid envelopes can accommodate themselves to this motion only by perpetual cracks and fissures, longitudinal and transversal, alternating with per-petual reunions by pressure, or by filtration and congelation.

The same theory of the internal condition of solids and fluids, accounts for a fact relating to the form of the earth, which other-wise seems difficult of explanation. What is the reason that being solid it takes the form which allows of a great portion of its surface to be covered to a comparatively small depth with a fluid? The answer which the theory gives to this question is, that the mass of the earth, taken as a whole, must be regarded as a fluid in the mathematical investigation of its form, and the rigidity of the superficial crust only accounts for local elevations and depressions, without having sensible influence on the general form. The effect of internal pressure would cause the distinction between solidity and fluidity to cease, probably at no great depth; and consequently any theoretical investigation which admits a dif-ference between solid and fluid parts at considerable depths below the surface, would seem to be inconsistent with the laws of mole-cular forces. For this reason also the explanation which the Astronomer Royal has offered of the anomalous deviation of the plumb-line in India, by making the special hypothesis that under

the Himalaya range a large solid mass is plunged into molten liquid of greater specific gravity, is liable to objection, unless it can be shown that the distinction between the solid and fluid states, and any difference of density, can exist under the pressure of the mountain mass, at the depth which the explanation requires. I have suggested a different explanation of the anomaly in the article on the Force of Gravity.

If this theory of the internal molecular condition of solids be true, there must be limits to the heights and acclivities of mountains, and to the depths of ocean-basins, depending on the energy of the superficial molecular attraction. The separation of large masses into parts by faults and fissures, by increasing the quantity of containing surfaces, probably renders a greater amount of superficial irregularity possible. If these irregularities and the effect of centrifugal force be disregarded, large bodies, like the sun and planets, would, according to the theory, take the form of a sphere. The form of a *very thin* plate, like that of Saturn's Rings, is also consistent with the theory : but it does not appear that any form very unlike these two would be possible.

5. Passing now to the consideration of the *gaseous* state of a substance, the first remark to make is, that since the mean interval between the atoms is much larger than in solids and fluids, the *atomic* repulsion, which varies very rapidly with distance, may be supposed to be of insensible magnitude. Also the condensations and rarefactions of the waves propagated from a single atom, so far as they are dependent on the number of atoms in a given space, will be much diminished in the aëriform state. At the same time the radius R of the spherical surface which includes the fixed number of atoms N, must be much greater, and the distance D at which the waves from the atoms merge into waves of the second order be proportionally increased. Hence the condensation and velocity in the waves of this order originating in the molecule of radius R, may never rise to such a magnitude as to satisfy the condition of producing excursions of the ætherial particles large compared to the diameter of an atom. Consequently they will be throughout waves of *repulsion,* until they merge into those which act as gravity. The tendency of aëriform substances to expand is in this manner accounted for by the theory.

By the aid of this theory, it is also conceivable that a gas, by being greatly compressed, so that its atoms are brought into such proximity that molecular *attraction* begins to act, may be converted into a liquid. Another property of gases is also simply explained by the theory, viz. the facility with which the atoms of one gas permeate another. The comparatively large intervals

between the atoms of a gas, would allow any diffusive action operating on the atoms of another gas, to take effect in the space occupied by the former, and all *collision* between the two sets of atoms would be prevented by the proper repulsions of the individual atoms.   It is evident that the force by which the atoms in motion are diffused, must act on the atoms of the medium in which the diffusion takes place; but this action only produces a slight alteration of the density of the latter, without affecting its state of equilibrium.

In applying the general expression for the velocity (V) of the æther along the surface of an atom, to account for the repulsive force of gases, it must in general be supposed that each of the constants μ and q has a sensible value, and consequently that both the hemispherical surfaces of the atom are pressed by the incident series of waves, the pressure on that which directly receives the waves predominating.   This may account for the comparatively small repulsive action of a gaseous body which is at no part extremely rare, such as the earth's atmosphere, at the upper boundary of which the force of the earth's gravity imposes a limit on its rarefaction.   But the repulsion will assume a different character in a gaseous substance of very large extent and great tenuity, such as was the coma of Donati's comet, which produced no sensible refraction of the light from stars, whatever were the direction and length of the path of the light through it.   In this case the dimensions of R and D are greatly extended; μ becomes large and q very small for waves of the second order, and their action on any atom approximates to that of the repulsion of the first order, extending but little beyond the hemispherical surface on which they are directly incident, while at the same time the action varies but slowly with distance. This will account for the enormous development of repulsive action in the extremely attenuated tails of comets.

6. The foregoing theory of molecular forces admits also of application to the following physical problems.

PROBLEM I. *To account for the difference of elasticity of different simple gases.*

The theory allows of no other difference between simple gases than a difference in the magnitudes of the component atoms. Take a portion of one gas bounded by a spherical surface, and containing a certain number (n) of atoms, and a portion of the other gas, bounded by an equal spherical surface, and containing the *same* number of atoms similarly arranged.   Let R be the radius of an atom of the former, and r the radius of an atom of the other, and suppose R to be greater than r.   Now by the theory, the elasticities of the gases depend on the waves accompanied by condensations which are reflected from the atoms; and

by hydrodynamics, the reflected condensations, the incident waves being the same, vary as the radii of the atoms. Hence the condensations at the *same* distances from the centres of the atoms are proportional to the *squares* of the radii. Hence also the resultant condensations from all the atoms at points P similarly situated with respect to the two spherical spaces, vary as the squares of the radii. Let now the number of atoms in that space which contains the smaller atoms be increased till it contains the same quantity of matter as the other space. Then the number of the smaller atoms will be $\dfrac{nR^3}{r^3}$, and the resultant condensation at P of the waves propagated from them will be increased in the same ratio. Hence the ratio of the resultant condensations due to the smaller atoms to the resultant condensations similarly due to the larger atoms will be $\dfrac{R^3}{r^3} \times \dfrac{r^2}{R^2}$, or $\dfrac{R}{r}$. And as, according to the theory, the forces which tend to produce motions of translation of the atoms, to which the pressures which counteract the expansions of the gases may be assumed to be proportional, vary as the squares of the condensations or velocities of the incident waves, it follows that *the pressures of equal weights of two simple gases contained in equal spaces vary inversely as the squares of the radii of their atoms.* Hence the elasticity of a simple gas is greater the smaller its atoms.

Problem II. *To find the relation between pressure and density in different substances.*

This question, which is the special subject of a communication to the Philosophical Magazine for June 1859, is introduced here for the purpose of answering it more completely by the application of the theory of molecular forces contained in the present communication. I see no reason to modify the argument by which the effective acceleration of an atom, due to the action of the surrounding atoms, may be expressed by the function

$$H \cdot \frac{\Delta \rho}{\Delta z},$$

$\Delta \rho$ being the increment of density corresponding to the linear increment Δz in the direction perpendicular to a surface of equal density, and therefore in the direction of the molecular acceleration. But it is to be observed that the factor H was obtained on the hypothesis that the sphere of molecular activity is not altered by change of density. According to the theory now advanced, it appears that, while upon an increase of density the action of the atoms in a *given* elementary space upon an atom at a *given* distance is increased in the same proportion, the sphere of molecular activity is *diminished* in that proportion, the linear

quantities on which the law of molecular action depends retaining the same ratios. It follows that the number of the given elementary spaces containing effective atoms is diminished in proportion as the density is increased, and consequently that the factor H is of the form $\dfrac{K}{\rho}$, the quantity $\dfrac{\Delta\rho}{\Delta z}$ being given. It is here supposed that atoms situated in parts of the fluid which have different densities have the same specific heat, that is, are centres of waves of equal condensation. But though this is approximately true in aëriform bodies, it cannot be exactly true, because, while the waves reflected from a given atom are chiefly due to incident waves from extraneous sources, they are partly due to secondary incident waves originating at the surrounding atoms, and therefore partly depend on the density. Hence, the reflected waves from extraneous sources being supposed to be the same throughout the fluid mass, we ought in the place of K to put $K\big(1+\alpha.f(\rho)\big)$, α being very small. Consequently if G be the accelerative force which counteracts the molecular acceleration of a given atom, we shall have

$$G=\frac{K}{\rho}\Big(1+\alpha.f(\rho)\Big)\cdot\frac{\Delta\rho}{\Delta z}$$

and

$$dp=G\rho\Delta z=K\big(1+\alpha.f(\rho)\big)\Delta\rho,$$

which is the law for gases.

Exactly the same kind of reasoning applies to fluids and solids, excepting that in these the condensations of the waves propagated from a given atom appear to be determined as to quantity by the reflexions of successive orders of secondary waves from the surrounding atoms. It would be in accordance with hydrodynamical principles to say that the dynamic effect of waves coming originally from extraneous sources (as the sun, the planets, and stars) is by these reflexions *multiplied* in a certain proportion to the density of the substance. Also among the primary waves to which the secondary waves are owing may be reckoned the waves of the second order previously considered, the condensations of which are proportional to the same density. Without at present discussing this point at greater length, I shall assume as an hypothesis that the calorific repulsion of solids and fluids is fully taken into account by supposing that $H=K\rho$. Consequently

$$G=\frac{K\rho}{\rho}\cdot\frac{\Delta\rho}{\Delta z}\ \text{ and }\ dp=G\rho\Delta z=K\rho\Delta\rho.$$

Hence

$$p=\frac{K}{2}\cdot\rho^2+C.$$

Problem III. *To find the law of density in the interior of the earth.*

This question is here considered for the purpose of testing the formula just obtained for the relation between the pressure and the density in solids and fluids, which is in a great degree hypothetical. To adapt the formula to the circumstances of the earth, supposed to be of homogeneous material throughout its interior, let

$$p = k^2 \left(\frac{\rho^2}{\delta^2} - 1 \right),$$

δ being the density of primitive rock, as granite, at the surface. Then, as is well known, this equation gives for the law of density in the earth's interior, the form being supposed spherical,

$$\frac{\rho}{D} = \frac{\sin Qr}{Qr},$$

ρ being the density at the distance r from the centre, D the density at the centre, and Q a certain constant. If c be the earth's radius, M its mass, and g the usual measure of gravity at the surface, the value of the constant Q is given by the equation

$$Q^2 = \frac{2 \pi g c^2 \delta^2}{M k^2}.$$

Now since the assumed relation between p and ρ takes into account the effect of heat in the earth's interior, the constant k^2 has the same value for the whole of the mass, supposed to be of uniform material. Hence its value may be found experimentally by determining the compressibility of primitive rock at the earth's surface, or, what is equivalent, ascertaining the velocity with which it transmits sound. (See on this subject an article "On the Ellipticity of the Planets," which I communicated to the *Philosophical Magazine* for September 1831, p. 200.) If this velocity be called V, by a known process we shall have

$$V^2 = \frac{2 k^2}{\delta}.$$

Hence supposing the mean density of the earth to be $n\delta$, we obtain for calculating Qc the equation

$$Qc = \frac{1}{V} \cdot \sqrt{\frac{3 g c}{n}}.$$

The value of n deduced from the law of density is 2·42. I am not acquainted with any experimental determination of the value of V for primitive rock. In art. 114 of the "Treatise on Sound" in the *Encyclopædia Metropolitana*, experiments which appear to be trustworthy are adduced which give for the velocity of sound

in cast iron 11090 feet per second.   Making use of this deter-
mination for want of one more appropriate to the problem, and
taking $c = 3956$ miles, it will be found that $Qc = 2,602$.   This
result comes very close to the value $\dfrac{5\pi}{6}$ or 2,618, which has been
assumed in the Theory of the Earth's Figure solely on the *à pos-
teriori* ground that it gives results in accordance with the observed
values of the earth's ellipticity and the precession of the equi-
noxes.   I have gone through the above investigation chiefly for
the purpose of making the remark, that the assumed relation
between the density and pressure, and the consequent law of the
earth's density, will in a great degree be shown to be physical
facts, if they bear the test of at once satisfying the observed
values of the ellipticity, the precession of the equinoxes, and the
rate of transmission of vibrations through the substance of the
earth's crust.   In the same degree, a theory of molecular forces
from which that relation may be antecedently deduced receives
confirmation.

   If it may be concluded from the foregoing arguments and ap-
plications of the results, that the theory of molecular forces here
proposed has a real foundation, what will be chiefly required to
complete it is to ascertain by analysis the precise composition of
the functions μ and q which have been so frequently referred to.

   Cambridge Observatory,
      January 18, 1860.

XIV. *Description of a New Seismometer constructed in the Col-
   lege at Monza.   By* P. G. M. CAVALLERI, *Professor of Physics
   at the Barnabite College of Monza**.

[With a Plate.]

To the Editors of the Philosophical Magazine and Journal.

   GENTLEMEN,

THE following memoir by Prof. Cavalleri of Monza, although
      in some respects behind the actual state of knowledge,
may not be unacceptable to English readers interested in seis-
mology.   From the unfortunate condition of Italy, the learned
of that country know commonly but little of what is doing
elsewhere in science; and our English libraries are miserably
supplied with Italian periodical literature.   Hence in 1858,
when in the "Fourth Report on Earthquakes" (Trans. Brit.
Association) I enumerated and discussed all the seismometers

   * Published at Milan, February 1858.   Extracted from the *Atti dell' I.
R. Instituto di Scienze, Litt. ed Arti*, vol. i. part 2.

known to me, those of Cavalleri I had not heard of, nor yet until recently I was indebted to M. Jeitelles of Kaschau in Hungary for a copy of the memoir. I have deemed it worthy of translation, partly as my *amende* to the author for my unintentional omission of him, and also from the fact that one of his arrangements, viz. the pendulums of variable length, is I believe new, and that he places some views relative to the movements of earth-waves in a clearer light to unmathematical readers than is usual.

I shall, with your permission, in a future Number of the Magazine make some remarks as to the limits of utility of the proposed instruments, derived from the experience of the actual phænomena of shock, obtained in my observation of the earthquake regions of the Two Sicilies early in 1858.

I am, Gentlemen, yours, &c.,

Monkstown, Co. Dublin,   ROBERT MALLET.
January 12, 1860.

Physical science, and geology especially, have long sought for a seismometer which should record the shocks and convulsions to which the surface of our globe is subject. The various theories which have been formed on the origin of earthquakes, and the nature of the strata beneath us, are founded on the various anomalies and particular effects which earthquakes produce in different localities. With the increase of data we have ascertained many things relating to physics and geology, many ideas have been rectified, and many new ones introduced. We have, indeed, several recent works which show a real advance and true scientific conquest. From the researches of Professor Alfonso Favre, made known to us in his two recent works on a hundred earthquakes which occurred in various parts of the world during the years 1855 and 1856, not to enumerate other works on the earthquakes of Calabria and Tuscany, it is evident that much light has been thrown on the nature of the waves of the terrestrial crust when agitated by earthquakes, on their variable rapidity and intensity according to the difference of the ground, on their refraction and interferences, on their inclination to extend along valleys, and in the direction of mountain chains rather than across them, and on many other questions no less new than interesting. But these ideas and views need much diligent investigation before any theory can be formed; and instruments of measurement are indispensable.

Although seismometers may afford us some facility for the advancement of science, and may aid in the investigation of these difficult questions, yet many, despairing of their success, have affirmed that the direct study of the phænomena of the earth-

quake, as exhibited in the effects which it has produced, is a far
superior method.   But besides the difficulty of giving, among a
mass of effects apparently contradictory, the due value to each,
we should remember that frequently the earthquake leaves no
distinct trace of direction, origin, or intensity, and still more
frequently *no trace* whatever.   And yet these weak and disre-
garded perturbations might afford precious data to science.   The
study of seismometers appears to me very useful, if it be possible
to add anything to what we have already done.   A *perfect*
seismometer should record the traces of the various motions
which affect the surface of the earth, at once marking their com-
mencement, duration, and relative intensity.   The formation of
one seismometer which should embrace these complex phæno-
mena being almost impossible, I perceived at the beginning of
my work that it was necessary to divide them into their com-
ponent parts.   Such a division being made, the invention of an
instrument which should record these components, or we might
say elements of the complex motion, became much easier.   I am
not unacquainted with the many seismometers made and pro-
posed by others, and have endeavoured to profit by their designs ;
but, none appearing to me satisfactory, I have changed and added
to their plans.   I subjoin a description of the apparatus which I
have constructed and recently tried.

A brass ball, weighing 3 kilogrammes, is vertically suspended
by a wire 1 millim. in diameter ; this wire, in length 1·25 metre,
is fastened at the point of suspension to the end of a strong
iron plate by means of a screw ; the plate is secured in the wall,
and projects horizontally from it 5·3 decimetres.   A needle is
firmly attached to the lower part of the ball with the point turned
down and finely sharpened ; in length it is 9 centimetres.   The
apparatus is in fact a pendulum.   The extreme point of the needle
is inserted a millimetre or a little more into an extremely small
cylinder or square prism (*viera*), but so as to be quite free.   This
cylinder rests lightly on the summit of a small support or vertical
column fixed in the centre of an iron pan which is firmly attached
to the wall.   The pan is filled with finely-sifted ashes or brickdust
to the level of the needle's point, or a little higher, as may be seen
in the accompanying Plate (Pl. I.).   The apparatus being arranged
in this manner, it is evident that if a shock occurs, let us sup-
pose from the south, the ball of the pendulum, owing to its own
inertia and the time necessary to transmit the motion from the
top of the wire to the ball itself, will remain unmoved ; while the
wall, the fixed plate which holds the pendulum, the pan, and
the support of the small cylinder will be pushed at the same
instant towards the north.   By the simultaneous motion of all
the rigid parts of the apparatus, the support of the vertical

column is withdrawn from beneath the small cylinder, which is retained in its position by the point of the needle; wanting the support, it will immediately fall into the soft ashes, precisely in the direction from which the first shock came, that is, from the south. This is the first effect of the seismometer, it will indicate the point from which the first shock came. The shock might occasion a very small movement; yet as we have the power of making the point extremely fine, and the cylinder and its support extremely small in diameter, it is evident that the slightest shocks would affect them, even those of two millimetres or less. From the experience of others, and from what has come under my own observation, it is certain that the shocks, although very weak, would cause a greater deviation than two or three millimetres. It may also be urged that the percussion occasioned by the passing of cars, or by thunder, might be capable of shaking the walls, and thus giving a fallacious indication of an earthquake. To this objection I beg leave to reply, that if the walls of the building be firm, and especially if the instrument be erected on the ground floor, these extraneous percussions can have no sensible influence on the walls, but are limited to the air, the window-panes, and such elastic and moveable objects as are placed between compressed and compressing air. Although passing wheels may communicate a motion to walls to which we may apply the word perceptible, it is in reality very slight, and almost invisible to the naked eye. For this reason I am led to believe that the instrument will not be in the least affected by passing cars, and will sufficiently answer our purpose.

I shall venture to dwell a little longer on this point, and allude to everything which may possibly influence our seismometer. Shocks of earthquake, however complicated, and as yet not submitted to measurement, may be distinguished, as is usually done, into undulatory and horizontal, subsultatory and vertical, and mixed shocks, the last being by far the most frequent. The apparatus which I shall attempt to describe will enable us to distinguish these three kinds of earth-waves. But there is still a question relative to the record of the direction of the primary wave by means of the small cylinder or prism. When a shock occurs in any given point of the globe, or rather when the centre of the earthquake is manifested in any given point, we know that undulations proceed from that point as from a centre, and are propagated over a circumference more or less extensive, according to the intensity of the shock or the conducting power of the ground. Now I would ask if the first wave, which is gradually extended and enlarged in its course, always proceeds from the centre of convulsion to the circumference, or whether the reverse can ever happen? Let us imagine the sudden crushing of a large hollow glass ball from

which the air has been exhausted; is it not evident that, at the moment of rupture, the surrounding air will rush violently in to fill the vacuum, and the first wave will come from the circumference to the centre; and that the same may be affirmed of the other more distant waves which successively enter the ball? On the other hand, should a certain quantity of gunpowder be ignited, in consequence of which a gas is formed which demands a new or greater space, will not the first wave proceed from the centre to the circumference? To make use of a better expression, we may call this last wave positive, and the first negative. This question might be theoretically treated by mathematicians, and their considerations might be of essential service in throwing light on the still obscure origin of earthquakes.

An earthquake may be produced from two very different causes with reference to our mode of considering the wave. It may happen that a considerable quantity of water or other matter may instantaneously produce such a volume of gas or vapour as will raise or displace in some manner a portion of the terrestrial crust and afterwards allow it to return to its primitive position; on this supposition the first wave must be positive. But if the steam or gas be slowly formed and expanded gradually (with increasing tension) till it finds instantaneous vent in the open crevices of the earth's crust, the first wave must be negative. In either case, laying aside the theoretical consideration of the ever difficult problem of the waves, of which we know neither the origin nor the depth, nor the medium through which they are propagated, nor the great and various pressure of the different strata, our pendulum may solve the practical question for us; for should we find, after a certain number of earthquakes of which the centre of effort has been subsequently ascertained, that our little cylinder had been displaced either in the direction of that centre or in the opposite direction, we might infer whether the earthquake had originated from the first or from the second of the supposed causes. Besides, we may conclude that the negative wave must rapidly decrease in strength as it is removed from the centre, and cannot be sensibly felt as far as the positive wave, which is in its nature much more powerful. Some experiments, although very imperfect, which were tried on the surface of a lake on which were placed wood floats, bearing card cylinders balanced so as to fall easily at the agitation of the water either in a positive or a negative sense, led me to conclude that the negative wave must be extremely weak. The accuracy of our instrument in noting the direction of the primary wave may throw greater light on another phænomenon, and perhaps completely solve it: I mean the phænomenon already noticed by some geologists, that the earth-waves produced by shocks take

certain directions in preference to others, according to the different lie of valleys and mountains; and further, that these waves are broken in their course and reach a given point more or less late, thus occasioning confluxes of different waves all generated from the primary, so as to produce shocks more considerable than at spots nearer the first convulsion.   This appears to be the necessary consequence of the heterogeneous material of which the crust of our globe is composed.   A single strong shock sent through strata of different density, arrangement, and elasticity, the strata also lying at angles widely varying from the direction of the primary impulse, and afterwards subject to different changes of dip (*incidenza*), must indubitably occasion—1st, a composition of forces; whence, 2nd, a different direction of waves; 3rdly and lastly, varying velocity in their progress.   These effects would of themselves be greatly complicated, even admitting that the waves should act only in one plane; but when we add, as truth requires, that the waves necessarily act in different planes according as the earthquakes are generated at a greater or less depth, and that the waves must radiate or expand, not over a single surface, but in a mass of three dimensions, every one must admit that the phænomena of earth-waves are most complicated, and have a thousand different aspects.   Our instrument is capable of recording these anomalies and others which we are about to notice.

When an earthquake occurs, the pendulum being disengaged from the cylinder, which falls in the direction from whence the first wave proceeded, is set at liberty, and traces in the ashes which lie beneath, and which rise a little above the point, the general direction of the wave. The application of the pendulum to tracing the direction of the earth-wave is already known, and has been frequently tried; I cannot boast of adding anything further to this invention than a most important auxiliary.   But here some observations present themselves which I consider very important.   The traces which the pendulum leaves impressed in the ashes, as I observed at Bologna during the earthquake which occurred there last year, are in general more or less long and well marked.   These are not occasioned by the oscillations of the pendulum itself, which are always relatively small, but by the ground moving under the pendulum.   The pendulum is soon set in motion, but its oscillations are very limited, and take the form of ellipses,—at first very excentric, almost pointed; but they shortly lose their excentricity and increase their minor axes, until they become small circles.   These figures are easily perceived if the point of the needle is sharp, and the ashes or brickdust smooth and regular, but only when the earth-wave proceeds from one direction. Should the waves come from two or more, it would

be impossible to make them out accurately. However, if we can-not always accurately tell the direction of the waves which have different horizontal inclinations, we can at least discover the direction of the principal one. Since the oscillations of the pendulum, especially if it be a long one, must be very small in comparison to the motion of the terrestrial crust lying beneath it, we hope no argument will be drawn thence against increasing the sphere of application of our seismometer. Another desideratum is to mark the time when the earthquake commences. In some seismometers which I saw at Bologna, and in others of which I have read, at the moment the shock occurs, the pendulum liberates a rod or a weight which in some manner (several methods are employed) stops the motion of a timepiece which is placed near. In others, a pencil moved by clockwork draws a line on a card divided into twenty-four parts, according to the hours of the day; an irregularity in the line will prove the occurrence of an earthquake. This manner of marking time has the advantage of compelling the observer to take daily note whether an earthquake has occurred or not; but is attended with the enormous inconvenience of requiring a person to attend to the instrument daily, perhaps for years, before the occurrence of the desired phænomenon. In our apparatus it is just the reverse. The timepiece, constructed with a main spring and a strong balance, and secured to the wall, is always wound up, but does not go. The instant a shock moves the pendulum, however slightly, a lever which retains the balance in a position favourable to its easy disengagement, is set at liberty, and the timepiece begins to mark time. The index is placed at zero, and can mark twenty-four hours, the dial being divided into twenty-four parts. It is evident that at whatever hour of the twenty-four a person perceives the motion of the clock or the displacement of the lever, or hears the ticking of the timepiece, &c., he can accurately tell at what hour the earthquake took place, by subtracting from the true time, as given by chronometers, the hours recorded by that which is attached to the seismometer. A whole day could not possibly elapse without the attention of some individual being attracted to the instrument, especially if (secured in a glass case) it were erected in a frequented and easily accessible place.

But seismometers ought to mark not only undulatory or horizontal motions, but subsultory or vertical also, as well as mixed ones. With regard to vertical upheavals, I have made use of a property which I observed belonged to spirals or elastic coils (*cliché*), viz. their power of vertical oscillation. I reflected that as the oscillations of the pendulum mark in a horizontal direction the horizontal undulations of the ground, so the vertical oscillations of the spiral might mark the vertical elevations of

the earth's crust. Let us imagine a spiral formed by an iron wire, hard as from the draw plate, with rings of equal size, forming a cylinder the spires of which are separated from each other. Let the spiral be attached at one extremity to a fixed point, suspended with its axis vertical, and let a moderate weight be placed at the other extremity. The spiral will lengthen and stretch a little, and then remain motionless. Now if the weight be pushed *up* and then left free, the spiral will oscillate like a common pendulum, only vertically. The elasticity of the spiral performs the same office as attraction in common pendulums. The descending weight acquires an accelerated motion, which tends to stretch the spiral more than it would do were it at rest; hence follows the reaction of the elastic spiral, which tends to draw the weight up more than it would do were it motionless; and this continues until, after a certain time, the resistance of the air and the imperfect elasticity of the spiral stop the vertical pendulum. Such is the apparatus which I have constructed for noting vertical upheavals or elevations of the earth's crust. But as the number of the oscillations of the spiral within a given time must depend on the weight which is attached, the size of the rings or turns, the thickness of the wire, and the number of the rings, I considered it necessary to institute a series of experiments in order to give to the spiral, conditions capable of fulfilling our intention, and thus to render complete a work which, so far as I am aware of, has not been done by another.

The following laws are the result of my experiments:—

1st. The vertical oscillations are isochronous.

2nd. With the same length of wire, the number of oscillations in a given time is in the inverse ratio to the diameter of the spiral.

3rd. The number of oscillations with the same number of rings is in the inverse ratio to the square root of the weights which stretch the spirals, subtracting the weight of the spiral itself, which acts as a weight and tends somewhat to retard the oscillations.

4th. The number of the oscillations in a given time is in inverse ratio to the square root of the number of rings or coils in the spiral.

5th. With the same weight, length of wire, and diameter of spiral, the number of oscillations is in the inverse ratio to the diameter of the wire.

The first and fifth of these laws agree fully with those discovered by Coulomb relative to the elasticity of torsion. The second (note being taken that the spirals used in my experiments are cylindrical) arises from the constant relation between the length of the wire and the number of rings in the spiral, and

must therefore substantially agree with the laws of the same
philosopher. The third would agree if the weight of the spiral
were nought, or might be so considered; but as one cannot
attach to the spiral a weight which would render the weight of
the spiral itself evanescent, since it would draw down the spiral too
much and cause it to lose its elasticity, so the weight of the spiral
has always a sensible effect, and tends to retard the oscillations:
some advantage is gained by the use of spirals of tempered steel.
The second law has no counterpart (*riscontro*) among those of
Coulomb, as it depends on conditions not found in twisted
threads, on which the laws of elasticity of torsion depend. With
the same wire and of the same length, spirals can be formed
which will have widely different oscillations by enlarging or nar-
rowing the diameter of the coils.

These laws ascertained, it is easy to find the conditions best
adapted to our purpose. The weight attached to the spiral must
be of a certain size in order to produce a strong reaction, and to
move the markers which we are about to describe. The oscilla-
tions ought to be slow, so that the time employed by the rising
and falling of the ground may not exceed that required to trans-
mit the motion from the top of the spiral to the weight itself.
I have therefore given to the spiral, measured along its axis, a
length of 80 centims., and attached to it a weight of 1·2 kilog.
The diameter of the cylindrical spiral is 5·3 centims. It con-
sists of ninety rings, and vibrates seconds: the diameter of the
wire is about 3 millims. I have constructed it in the following
manner:—A strong iron bar fixed in the wall supports one end of
the spiral; a cylindrical weight of equal diameter to the spiral
is attached to the other extremity. This weight oscillates freely
within an iron ring secured to the wall. The spiral is enclosed
in a kind of cylinder in which it can freely oscillate vertically,
but not horizontally. The weight terminates in a point, and
rests on the short arm of a lever very easily moved; the other
arm of the lever, by means of a graduated quadrant, serves as
an index. All this apparatus of lever, quadrant, and index, is
securely attached to the wall, and is united to the bar by which
the spiral is suspended. Now let us suppose that the ground
has been suddenly elevated by an earthquake. It is evident
that, when the wall and the bar which holds the spiral are raised,
the arm of the lever, being part of the same rigid common system,
will rise also. But for a certain time the weight attached to the
spiral will remain unmoved in its place, because a sensible time
is necessary to communicate motion from the top of the spiral to
the weight itself. Meantime the short arm of the lever will be
pushed by the weight, and being very light, it will fall, while
the longer arm will rise and record the elevation. This arm is

ratcheted in such a way that it can easily go up, but cannot descend, as seen in the figure (Plate I.). Thus the index will mark the elevation of the ground or vertical undulations, the ratio between the two arms of the lever being taken into account. Instead of the lever we may use a cylinder of cork a little larger than the weight, which cylinder must run easily between two fixed vertical side-pieces or gùides, as represented in the figure. As far as the cork, by the push of the weight, has been lowered it will remain *fixed* there, being light and held by its own elasticity so fast that it is impossible for a shock alone to move it from its position. An upright scale, suitably divided, placed at the side of the cylinder, shows the height of the vertical wave,— as *it* rises with the ground, while the cylinder is depressed. This last method is, I think, superior to that which I constructed with the lever.

This instrument is so delicate, that by giving, if necessary, a larger diameter and a greater length to the spiral, and a corresponding weight, we may succeed in marking any slight elevation of the soil performed in a relatively long period of time. For example, we might record an elevation with a velocity of a millimetre per second, or even still slower. Knowing by observation that elevations or depressions of the soil occupy a very short time, I thought it useless to give the spiral a longer time of oscillation. It may happen that the first vertical wave is one of depression and not elevation; but in this case also the instrument will accurately note the vertical movement, in consequence of the elevation which succeeds the depression. It may happen also, and in fact often does, that the weight gradually stretches down the spiral and falls a certain degree (by loss of elasticity, namely); but in this case the short arm of the lever is carried down also by the weight, and the other arm the place fixes, on account of the ratcheting already mentioned, so that the space found between the weight and the lever below it will always indicate the height of the vertical wave. I will not conceal an objection which may be alleged against this method of measuring vertical undulations—an objection which, with much greater reason, may be raised to other seismometers,—viz. that when vertical undulations are frequently repeated, the instrument will mark sometimes more and sometimes less than the real altitude of the earth-waves, according as the time of the undulations is tautochronous with that of the oscillations of the pendulum or the contrary. If the spiral perform its oscillations more slowly, this defect will generally be very small; and besides, these vertical shocks being almost always confined to a single pulse, the instrument will mark accurately in most instances.

Lastly, there remain mixed shocks, *i. e.* those which, besides

moving the ground with the objects upon it in a horizontal direction, also elevate or depress it. These waves, which for perspicuity we have called mixed, are perhaps in reality the only ones which exist. Several authors, especially geologists, treat of these waves,—some from one point of view, some from another, and many so hastily that it may truly be said we are yet far from possessing a theory of their nature. Such at least is the impression received from the papers of Gay-Lussac, Humboldt, Achille Rossi, Savi, Collegno, Pilla, Favre, and more particularly of Dr. Young, who compares (I use his own words) "the earthquake of land to the earthquake of the air." He also compares the shock to the striking of a number of balls placed in a right line, of which, when the first is struck, the last only is separated. We can do no better, therefore, than closely to adhere to observation, and patiently record the components of these waves in order to draw deductions from them. In fact, having the vertical altitude of the wave recorded by the spiral, and the horizontal deviation marked by the pendulum, we possess all the necessary data for mixed waves. Acting thus at right angles (leaving aside for the present more subtile considerations), we may regard the mixed wave as the diagonal of these two forces, so that the mixed wave would be equal to the square root of the sum of the squares of the above-named components. Besides, we might also get the inclination which the plane of the mixed wave makes with the horizon by considering this diagonal as radius, and the vertical altitude given by the spiral as the sine, the angle of inclination being that corresponding to this sine. A good seismometer ought also to mark the time of the duration (*tempo della durata*) of the shock; but although I do not think it impossible to form such an arrangement as should mark this also, the problem appears so complicated and difficult that I am not at present prepared to attempt its solution. I have rather sought to discover the time which the earth-wave employs in its excursion, or that of the seismometrical oscillation. It is desirable to learn how many undulations the earth-wave makes in a given time, and thence to see if this do not differ in every instance of earthquake and in every country, on account of the different strata through which the wave is transmitted, and perhaps also on account of the diversity of originating causes. Earthquake-waves, so far as I can remember in three distinct cases which I have present to my mind, appeared very rapid and almost isochronous. Recalling these shocks, it does not appear to me far from the truth to assign about three undulations per second as the rate, at least in our Lombardy Plains. The instrument which I have constructed with this view will note the duration of these undulations, and consequently whether they are different

in various countries and in different earthquakes. The principle upon which the instrument is constructed is very simple.

Let us imagine a pendulum formed by a ball suspended by a wire, and this wire attached to a moveable point of suspension. If this point of suspension be moved forward and backward a certain distance in the same horizontal line, moving to and fro in equal times with the time of oscillation of the pendulum itself, then the arc which the pendulum makes continually increases with the increase of the motion to and fro of the point of suspension. If, however, the motion of this point occur in times which are not isochronous with the oscillations of the pendulum itself, its arc of oscillation will become less. In the first case, to the momentum which the pendulum acquires from being displaced by following the altered perpendicular of the point of suspension, must be *added* the momentum communicated by its moving to and fro, and so the arc of oscillation is increased. In the other case the force of the moving to and fro is partly or entirely *subtracted* from the vibrations of the pendulum, since it acts more or less in a contrary direction ; hence the arc of vibration is diminished. A reciprocating motion tautochronous with the pendulum causes the greatest arc of oscillation. These considerations being premised, I take a strong bar inclined some degrees towards the horizon. To this bar, and at such distances as shall exceed the amplitude of the largest earth-wave occurring, I fasten a number of small pendulums, as represented in the Plate. These pendulums terminate in sharp needles which touch the ashes beneath them, so that when a shock occurs they leave impressed the traces of their vibrations. The ashes are hollowed out like a cup, in order to assimilate to the arcs traced by the various pendulums, and prevent the deep impression of the needles, which might partly hinder their vibrations. The lower extremities of the pendulums form a horizontal line, as in the figure, so that their lengths vary : they are ten in number, which I consider sufficient. I have arranged them in such a manner that the shortest will make a little more than four oscillations per second, and the longest two. These two limits, of about four and two oscillations per second, appear to me sufficient to embrace every undulation occasioned by any earthquake. Let us suppose the pendulum to be set in motion by a shock. All the pendulums will vibrate, and leave separately traces of their oscillations impressed on the ashes ; and *the pendulum which has marked the largest arc will have performed its oscillations in equal times with those of the earth-wave* ; so that by finding the square root of the length of the pendulum, or, better, by actually observing its time of oscillation, we shall learn the duration of the undulations of that particular shock and in that

locality. I must add that usually, if not always, the undulations • are several in number, and therefore the increased force which the tautochronous pendulum acquires will always cause it to make an arc of oscillation visibly larger than the others, so that no doubt can arise as to which of the pendulums is that according with the motion of the earth-wave; but strictly, a single tautochronous wave would be quite sufficient. For clearness, we have here assumed the terrestrial wave to be horizontal; usually, if not always, however, it is mixed, and inclined more or less to the horizon. However, in this case it is obvious that, if we separate the mixed wave into its horizontal and vertical components, the latter being perpendicular to the horizon, can have no influence on the arc of oscillation of the pendulum, whilst the horizontal force remains alone active. The knowledge of the velocity of earth-waves, besides determining the relative intensity of earthquakes, may lead to most valuable discoveries, and explain many phænomena which now excite strong interest. I think that, having gained this knowledge by the aid of our seismometer, we possess the necessary data for calculating the intensity of the shock or earth-wave. In short, the intensity of the wave may be represented by three distinct elements; viz. the vertical altitude which the ground attains, the horizontal lengths of the wave, and the time occupied by this simultaneous movement. On this supposition, the intensity of the wave may be considered in the direct ratio of the two first quantities, and in the inverse ratio of the time. Now our instrument being capable of marking, 1st, the vertical altitude of the wave by the spiral pendulum; 2nd, the horizontal undulation by the great pendulum; 3rd, the time of the wave as marked by one or other of the small pendulums, we have all the elements necessary for calculating the intensity of the shock. Lastly, it is clear that with these three elements we can make all possible theoretical inferences, and assign to each of the three its appropriate value in referring to the effects of an earthquake, whether on buildings, on plains, or on the sea, &c., in all of which one or other of the three mentioned powers will have a greater or less influence : it has been proved, for example, that with an equal degree of intensity, the vertical shock will do more damage than the horizontal. Thus we can note with these instruments—

1st. The moment at which the earthquake occurs.

2nd. The direction of the primary shock or earth-wave.

3rd. The general horizontal direction of the waves, their amplitude, or length.

4th. The height of the vertical wave of shock, however complex the vertical and horizontal waves acting together may be.

5th. The resultant of both these elements, or the mixed shock itself.

6th. The inclination to the horizon of the mixed shock.

7th. The velocity and time of the wave.

8th. The total intensity of the wave, introducing into it the element of time as furnished by the pendulums.

If, then, as frequently happens, we also know the total duration of the earthquake, we may approximately infer its total intensity. I say approximately, because it appears that the last undulations are always weaker than the first or following ones. Were it not for this, we might obtain the intensity due to any given earthquake by multiplying the intensity of one wave, as above, by the number of the oscillations which were made by the pendulum in the total time the earthquake lasted. We shall conclude the description of this apparatus with a remark equally applicable to the other seismometers, which we purpose briefly to notice.

In earthquake convulsions, very irregular or rotatory perturbations occasionally occur. Our seismometer, although it is incapable of noting all, can record the most important of these convulsions, and intimate the occurrence of others by the irregular marks which will be impressed on the ashes. Lastly, wherever the centre of effort may be, the ground must be so affected as to destroy the apparatus, if happening to be there set up. This, however, will not occur once in a thousand times; so that out of a thousand instances the instrument will be of service in 999, as may be seen in the repeated shocks which occurred in July 1855, at Vispe in the Alps, as observed and described by Favre.

To complete my remarks on seismometers, it is necessary to institute a comparison of our seismometer with those that have come under my notice. Passing over some which do not deserve the name, such as the vessel of water with *floating particles* (*una polvere galleggiante*) which is spilled by the earthquake, we shall mention one attributed to Cacciatore, but really invented by a Milanese, as I was assured by the astronomer Carlini.

* * * * * * * * *

The remainder of Professor Cavalleri's memoir is occupied with a detailed description and discussion of the defects, &c. of the seismometer of Cacciatore (which he states has received important improvements by Coulier, but the nature of which Signor Cavalleri had not learnt); of Kreil; of those suggested by myself (as extemporaneous instruments only), in the first edition of the 'Admiralty Manual;' and of that of Prof. James Forbes.

As these have, however, all been more carefully described, and

their principles and disadvantages pointed out by myself in the discussion of the Seismic Catalogue of the British Association ("Fourth Report on the Facts and Theory of Earthquakes," Trans. Brit. Ass. 1858), it is unnecessary to occupy the English reader with the conclusion of the memoir, except to give the explanation by the author of his diagram.—R. M.

EXPLANATION OF THE PLATE.

A. Strong iron bar fixed in the wall.
B. Moveable disc in contact with the arm of lever.
C. Pan of fine ashes or brickdust.
D. Small cylinder or prism (*viera*) resting on
E. Metallic column fixed to the pan, in order to show the direction of the primary wave.
F. Two arms of a lever, the longest of which rests on the pendulum B, and the shortest acts as detent to a timepiece to prevent it going.
G. Timepiece with spring and balance always wound up, but not going, until the earthquake moves the small arm of the lever which acts as detent to the balance.
H. Bar fixed in the wall supporting the spiral.
I. Wire spiral, marking the vertical motion of the earth-wave.
L. Tube, or guides (*regoli*), which do not allow the spiral to oscillate horizontally, but only vertically.
M. Iron bar fixed in the wall, bearing a ring in which
N. The weight which stretches the spiral can move freely.
O. Lever, the small arm of which is lowered by the vertical undulation, and the long arm raised; and on account of the ratcheting it cannot fall again, and so indicates the height of the vertical wave.
P. Cork cylinder running freely by its own elasticity between two side guides (*regoli*). This cylinder is placed under the weight of the spiral, and being lowered by the motion of the ground or vertical wave, and remaining where it is driven by the weight, indicates the height of the vertical wave.
Q. Needle of the large pendulum (actual size), with the small cylinder and part of the little column which supports it.
R. Small pendulums on a graduated scale, as to length, to note the time of the earth-wave.

XV. *Chemical Notices from Foreign Journals. By* E. ATKINSON, *Ph.D.,F.C.S., Teacher of Physical Science in Cheltenham College.*

[Continued from p. 52.]

FITTIG [*] has investigated several processes of decomposition of acetone. When sodium is added to acetone, the liquid becomes filled with white flakes, and is ultimately converted into a lustrous, gelatinous substance, without the disengagement of any permanent gas. On subjecting the mass to distillation, a yellowish viscous oil was obtained which solidified to a crystalline mass. This mass, freed by pressure between paper from some adherent oil, and crystallized from a small quantity of boiling water, yielded

[*] Liebig's *Annalen*, April 1859.

large transparent quadratic plates. They contain water of crystallization, and even by pressing between paper partially lose their transparency. Their composition was found to be $C^6 H^{12} O^8$: Fittig considers that the body is an isomeric modification of acetone, the rational formula being $C^6 H^6 O^2 + 6$ aq. The crystals gradually lose water when exposed to the air, and more rapidly *in vacuo* over sulphuric acid; but it was impossible to determine this loss exactly, owing to the volatilization of a portion of the substance. That the substance was formed from the decomposition of acetone, and not from the crystallization from water, was proved by the fact that when some of the crystalline mass was pressed and crystallized from anhydrous ether, the crystals formed had the same composition.

Fittig further examined the action of caustic lime on acetone. Well-burned marble was covered with acetone, and left in closed vessels for some time; the dry yellowish mass was then distilled. On rectifying the distillate, it was found to consist of two bodies, of which one distilled below 150° and the other above 200°. By fractionally distilling the first of these a body was obtained which boiled at 131°·5, and in its analyses and properties was found to be identical with Kane's oxide of mesityle or mesitic ether, $C^{12} H^{10} O^2$. It is a colourless transparent oil, smelling like peppermint, and with a caustic taste. It burns with a lustrous flame, and is not soluble in water, but readily so in ether and alcohol. Oxide of mesityle is converted into a resin by the action of nitric acid, and with chlorine it yields a substitution product.

The other body produced by the action of caustic lime, was found to be partially decomposed by distillation. The analysis of a specimen gave results agreeing with the formula $C^{18} H^{14} O^2$. Hence it might be formed from 3 atoms of acetone with the elimination of 4 atoms of water. It is isomeric with phorone[*]; and in a subsequent investigation[†] Fittig found that it was identical with that substance. The body from acetone, by treatment with anhydrous phosphoric acid, yielded cumole, $C^{18} H^{12}$, from which, by oxidation with nitric acid, nitrobenzoic acid,

$$C^{14} H^5 (NO^4) O^4,$$
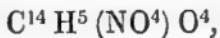
was obtained,—a result interesting as showing that from acetic acid, a member of the fatty acid series, a derivative of benzoic acid, a member of the aromatic acid series may be obtained.

The action of sulphuric acid and of alkalies on acetone appears to be identical, giving rise to the formation of a series of bodies which are acetone minus water, as is seen from the list.

[*] Phil. Mag. vol. xiii. p. 188.
[†] Liebig's *Annalen*, December 1859.

		Boiling-point.
Oxide of mesityle .	$C^{12} H^{10} O^2 = 2$ Acetone $-2HO$	$131°.5$
Phorone ? . .	$C^{18} H^{14} O^2 = 3$ Acetone $-4HO$	210
Mesitylene . .	$C^{18} H^{12}\ \ \ \ = 3$ Acetone $-6HO$	155
Xylite Naphtha .	$C^{24} H^{22} O^6 = 4$ Acetone $-2HO$	119
Xylite oil . .	$C^{24} H^{18} O^2 = 4$ Acetone $-6HO$	200

Chlorine acts on acetone in diffused light with great energy.
The product of the action was washed, dried, and distilled. On
rectification its boiling-point was found to be 120° C. Its
analysis and the determination of its vapour-density give for its
composition the relation $C^6 H^4 Cl^2 O^2$, which is that of Kane's
Mesitchloral. Fittig considers it to be bichlorinated acetone.
That it belongs to the acetone type, is evident from its forming a
crystalline compound with bisulphite of soda. It is a colourless
liquid with a penetrating odour, and strongly affects the eyes.
It has an extremely caustic action on the skin. It is insoluble
in water, but dissolves in alcohol and ether.

By the action of nitric acid on acetone a heavy yellowish oil
is obtained, which from its ready decomposability and explosive-
ness appears to be a nitro-compound. Its properties precluded
any accurate examination.

Fittig has further * examined the products of the destructive
distillation of acetates. He finds that acetone is not the only
product, but that other allied substances are formed at the same
time. In their separation he found it most convenient to use the
oil which floats on the surface of the crude acetone, in its pre-
paration. He succeeded in isolating the following bodies :—

Methylacetone, $C^8 H^8 O^2$, a colourless liquid resembling acetone,
but boiling between 75° and 77°.

Ethylacetone, $C^{10} H^{10} O^2$, a colourless liquid also like acetone,
and boiling between 90° and 95°. Both these bodies form cry-
stallized compounds with bisulphite of soda.

Dumasine.—This substance was first discovered by Kane, who
assigned to it the formula $C^{10} H^8 O$. It is a colourless liquid,
which, however, gradually becomes yellow. It is lighter than, and
insoluble in water, but quite soluble in alcohol. Fittig's analysis
gives for it the formula $C^{12} H^{10} O^2$. It is isomeric with oxide
of mesityle, but is distinguished by forming a crystallized com-
pound with bisulphite of soda, which oxide of mesityle does
not. Dumasine forms a chlorinated substitution product,
$C^{12} H^8 Cl^2 O^2$.

Städeler† has published an investigation of acetone, the greater
part of which consists of the details of experiments the results of
which have been already announced.

* Liebig's *Annalen,* April 1859.   † Ibid. Sept. 1859.

He has examined the body discovered by Fittig; he prepared it by the same method, and his description of its properties agrees with that of Fittig. He finds that, according to the conditions of its crystallization, it forms either long prismatic needles or thick plates. On account of its property of crystallizing in large plates, he names it *pinakone* ($\pi\iota\nu\alpha\xi$, *a plate*). Städeler's analyses of the body lead to the formula $C^{12}H^{12}O^2 + 14HO$. He represents it as formed from acetone by the loss of oxygen in the following manner:—

$$2(C^6H^6O^2) + 2Na = 2NaO + C^{12}H^{10}O^2 + 2H$$
Acetone.   Oxide of
           mesitile.

$$2(C^6H^6O^2) + 2H = 2HO + C^{12}H^{12}O^2$$
Acetone.   Pinakone.
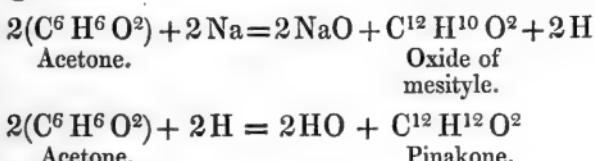
The soda formed at the same time decomposes some acetone, producing several oily bodies which distil over with pinakone, and among which appears to be phorone.

Schwanert* has investigated the action of pentachloride of phosphorus on hippuric acid. At ordinary temperatures there is no action; but when the mixture is gently warmed, hydrochloric acid is evolved, and a liquid distillate is obtained. The decomposition differs according to the proportions taken. When a mixture of one atom of hippuric acid with two atoms of pentachloride is distilled in small quantities, oxychloride of phosphorus first passes over, followed by a colourless oily liquid which distils between 180°—200° C.; and at length a liquid passes over between 200° and 250°, which partially crystallizes. In the retort a solid insoluble mass is left. The distillate between 180° —200° consists of chloride of benzoyle, containing a small quantity of crystals which have the composition $C^{18}H^6ClNO^2$: the crystals which distil over in the reaction consist chiefly of this substance. When pure they are colourless prisms, which fuse at 45°, distil at about 220° C., and crystallize on solidifying. They are distinguished by their great stability, which renders their investigation very difficult. Caustic potash is without action upon them. They form with hydrochloric acid a crystalline compound, which readily gives up hydrochloric acid. Besides this body there is formed in the reaction a small quantity of another crystalline compound, which seems to be $C^{18}H^5Cl^2NO^2$. They are both probably substitution products of a compound, $C^{18}H^7NO^2$.

Anhydrous sulphuric acid acts on hippuric acid, forming a clear brown solution. This is mixed with water, nearly neutral-

ized with carbonate of lead, filtered, the filtrate decomposed with sulphuretted hydrogen, and the filtrate from this carefully evaporated. A brownish yellow hygroscopic mass remains, which is *sulphohippuric acid.* Its formation is thus expressed:

$$C^{18} H^9 NO^6 + S^2 O^6 = C^{18} H^9 NS^2 O^{12}.$$
Hippuric acid.   Sulphohippuric acid.
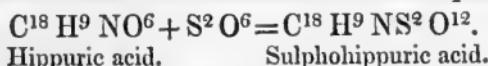
Sulphohippurate of baryta, formed by treating the carbonate with the acid, is bibasic, and has the formula

$$C^{18} H^7 Ba^2 NS^2 O^{12} + 2HO.$$

Sulphohippuric acid is decomposed by nitrous acid into sulphobenzoic acid and glycolic acid.

Nitrohippuric acid, $C^{18} H^8 (NO^4) NO^6$, formed by the action of sulphuric and nitric acids on hippuric acid, crystallizes in fine white needles. By treatment with sulphuretted hydrogen this body is converted into *amidohippuric acid,* $C^{18} H^8 (NH^2) NO^6$. It crystallizes from alcohol in light colourless laminæ. It is difficultly soluble in ether, but readily so in boiling water and alcohol. Its solutions soon become coloured on standing.

Schiel* has investigated the action of chlorous acid on certain organic substances. He found that chlorite of lead, which can be readily procured in large quantities, is a convenient form of using this reagent. When 30 or 40 grms. of chlorite of lead were mixed with about two-thirds the weight of alcohol, and a few drops of sulphuric acid added, the mixture soon became coloured yellow from chlorous acid; but when exposed to the sun, this colour disappeared. Sulphuric acid was added from time to time until the chlorite was quite decomposed: the liquid product of the reaction was found on rectification to consist of acetic ether. Its formation may be thus expressed:

$$2 \left.\begin{matrix} C^4 H^5 \\ H \end{matrix}\right\} O^2 + ClO^3 = \left.\begin{matrix} C^4 H^5 \\ (C^4 H^3 O^2) \end{matrix}\right\} O^2 + 3HO + HCl.$$
Alcohol.   Chlorous   Acetic ether.
                        acid.
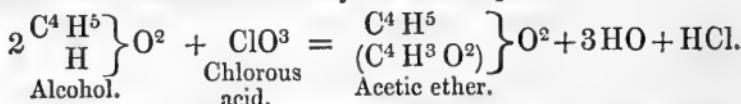
Amylic alcohol, treated in like manner, yielded valerianate of amyle.

By the action of an aqueous solution of chlorous acid on urea, a body was obtained crystallizing in large flat prisms, which were very hygroscopic. Its composition was found to be $C^2 H^8 N^3 Cl O^2$. It might be regarded as a compound of urea and sal-ammoniac, $C^2 H^4 N^2 O^2 + N H^4 Cl$. By crystallizing together equivalents of these substances, this body could not be obtained.

By the action of aqueous chlorous acid on uric acid, a new acid was obtained crystallizing in pearly laminæ, which formed

crystalline salts with baryta and lead, and with silver a caseous precipitate. Its composition is $C^{14} H^{11} N^6 O^{11}$; and Schiel names it *chloraluric acid.* Besides this, other bodies are formed, the investigation of which is not complete.

The gas in the dark cone of the non-luminous flame of Bunsen's gas-burner is a mixture of atmospheric air and coal-gas. Lunge* has analysed this gas with a view to ascertain the proportions in which these two constituents exist. To collect the gas, the following method was adopted :—In the upper part of the burner, a few millimetres below the mouth, a small aperture was made through which a fine, bent glass tube was so introduced that it stood exactly in the middle of the aperture of the burner, and projected about 17 millims. above. The diameter of this tube was about 0·3 millim.; it was fastened in the burner by means of gypsum, and its other end was connected by means of caoutchouc with two wide gas-collecting tubes. The caoutchouc junctions could be closed by means of Mohr's stopcocks. The last gas-tube was connected with an aspirator under a constant pressure, the efflux of which could be regulated by a tap.

The flame was so regulated that its height was about 136 millims.; the internal cone was about 61 millims. high. The aspirator was then set in motion, and a stream of gas withdrawn so slowly that the dimensions of the flame were scarcely altered. When the tubes were full they were closed, and transferred to a eudiometer.

The analysis of the gas, made according to the methods described in Bunsen's gasometric methods, gave the following results for the composition of the mixture :—

Carbonic acid.	0·00
Oxygen	14·28
Elayle	1·67
Ditetryle	0·76
Carbonic oxide	2·73
Hydrogen.	11·94
Marsh-gas	12·97
Nitrogen	55·65
	100·00

Earlier analyses of the Heidelberg gas (which was used in this investigation), as well as some analyses made simultaneously with this research, showed that it contained no oxygen; and hence all the oxygen found could be assigned to the atmospheric air of the mixture, the quantity of which was accordingly 68·13

per cent. There remains, therefore, 31·78 per cent. gas of the following composition :—

Carbonic acid	0·00
Oxygen	0·00
Elayle	5·24
Ditetryle	2·38
Carbonic oxide	8·58
Hydrogen	37·46
Marsh-gas	40·70
Nitrogen	5·64
	100·00

This composition agrees with other analyses of Heidelberg gas, and from it may be calculated the quantity of oxygen (*i. e.* air) necessary for the perfect combustion of the gas which must reach it from the outside. For—

1·67 vol. Elayle requires	5·01 vol. oxygen.
0·76 „ Ditetryle „	4·56 „
2·73 „ Carbonic oxide	1·37 „
11·94 „ Hydrogen „	5·97 „
12·97 „ Marsh-gas „	25·94 „
	42·85

From this it appears that 100 parts of this gas mixture still require $42\cdot85 - 14\cdot28 = 28\cdot57$ parts of oxygen, which correspond to 136·30 parts of air. Hence in the flame of this burner, almost exactly one-third of the oxygen (*i. e.* air) necessary for complete combustion reaches it from the interior.

Lunge has also calculated, according to the methods described in Bunsen's work, the temperature of the flame for this gas. He finds that it is 2781° C.

In the expectation of forming oxide of ethylene[*] directly from glycol in accordance with the following equation, Lourenço[†] heated together glycol and bromide of ethylene :

$$\ddagger 3\left(\begin{matrix}C^2\,H^4\\H^2\end{matrix}\right\}\Theta^2\right) + C^2\,H^4\,Br^2 = 2\left(\begin{matrix}C^2\,H^4\\H\end{matrix}\right\}\Theta\right) + 2C^2\,H^4\,\Theta + 2H^2\,\Theta.$$

Glycol. Bromide of Br Oxide of
ethylene. Hydrobromic ethylene.
glycol.
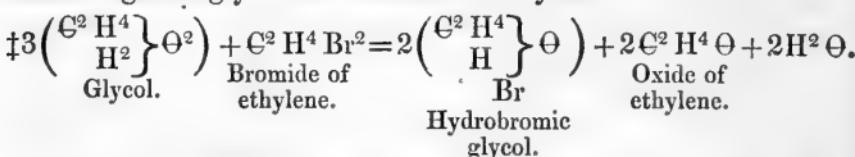
Hydrobromic glycol and water were formed; but instead of oxide of ethylene, a substance is obtained boiling above 230° C., having a sweet taste, the consistence of glycerine, and perfectly

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[*] Phil. Mag. vol. xvii. p. 427.
[†] *Bulletin de la Soc. Chimique,* p. 77.
[‡] $C=12$; $\Theta=16$; $C=6$; $O=8$.

soluble in water, alcohol, and ether. Repeated analyses and a vapour-density determination gave for it the formula $C^4 H^{10} O^3$, and its formation may be thus expressed :—

$$3\left(\begin{array}{c}C^2 H^4 \\ H^2\end{array}\right\}O^2\right) + C^2 H^4 Br^2 = 2\left(\begin{array}{c}C^2 H^4 \\ H\end{array}\right\}O\right) + C^4 H^{10} O^3 + H^2 O.$$

Glycol. Bromide of ethylene. Br New body.

Hydrobromic glycol.
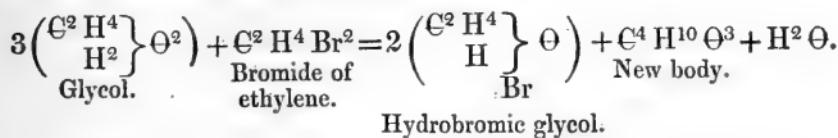
In its composition it is intermediate between glycol and the ether of glycol; it may be represented as two molecules of glycol united, with the elimination of an atom of water :

$$\left.\begin{array}{c}\left.\begin{array}{c}C^2 H^4 \\ H^2\end{array}\right\}O^2 \\ \left.\begin{array}{c}C^2 H^4 \\ H^2\end{array}\right\}O^2\end{array}\right\} = H^2 O + \left.\begin{array}{c}C^2 H^4 \\ C^2 H^4 \\ H^2\end{array}\right\}O^3.$$
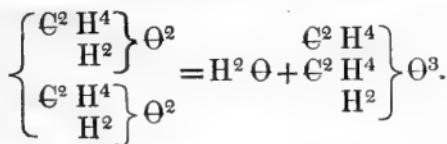
Wurtz[*] has published some additional researches on oxide of ethylene. Oxide of ethylene has the properties of a base; it combines directly with hydrochloric acid to form hydrochloric glycol :

$$HCl + C^2 H^4 O = C^2 H^4 HCl O = \left.\begin{array}{c}C^2 H^4 \\ H \\ Cl\end{array}\right\}O.$$

Oxide of ethylene. Hydrochloric glycol.
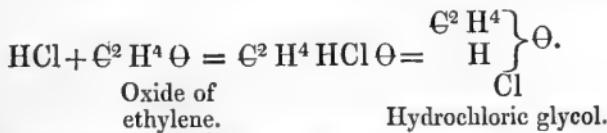
It also combines directly with acetic acid to form the acetate of glycol.

It combines with water to regenerate glycol. The two substances are heated together in a sealed tube. The product of the action has a saccharine taste. On distillation, glycol first passes over, and the temperature rises to about 210°. The distillate then consists of the intermediate ether of Lourenço above described. Its formation and that of glycol are expressed by the reactions :

$$C^2 H^4 O + H^2 O = C^2 H^6 O^2.$$
Oxide of ethylene. Glycol.

$$2(C^2 H^4 O) + H^2 O = C^4 H^{10} O^3.$$
Oxide of ethylene. Intermediate ether.
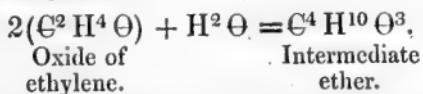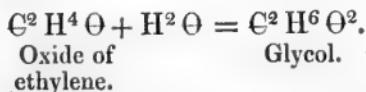
Wurtz points out that this intermediate ether bears to glycol and oxide of ethylene the same relations as Pelouze's anhydrous lactic acid does to lactic acid, and to lactide.

$$\left.\begin{array}{l}\mathrm{C^2\,H}''\\ \mathrm{C^2\,H^{4\prime\prime}}\\ \mathrm{H^2}\end{array}\right\}\Theta^3 \qquad\qquad \left.\begin{array}{l}\mathrm{C^3\,H^4\,\Theta}''\\ \mathrm{C^3\,H^4\,\Theta}''\\ \mathrm{H^2}\end{array}\right\}\Theta^3.$$

<div style="text-align:center">Intermediate   Pelouze's anhydrous
ether.   lactic acid.</div>

$$\left.\begin{array}{l}\mathrm{C^2\,H^{4\prime\prime}}\\ \mathrm{H^2}\end{array}\right\}\Theta^2, \qquad\qquad \left.\begin{array}{l}\mathrm{C^3\,H^4\,\Theta}''\\ \mathrm{H^2}\end{array}\right\}\Theta^2.$$

<div style="text-align:center">Glycol.   Lactic acid.</div>

$$\mathrm{C^2\,H^{4\prime\prime}\,\Theta} \qquad\qquad \mathrm{C^3 H^4\,\Theta''.\,\Theta.}$$

<div style="text-align:center">Oxide of ethylene.   Lactide.</div>

Oxide of ethylene also* unites with glycol under the same circumstances as with water. The principal product of the reaction is the above intermediate ether, $\mathrm{C^4\,H^{10}\,\Theta^3}$; but when this has distilled over, a very thick colourless liquid is obtained which boils at about 290°. It is formed by the combination of two atoms of oxide of ethylene with one atom of glycol, and its composition is expressed by the formula

$$2(\mathrm{C^2\,H^4\,\Theta}) \;+\; \mathrm{C^2\,H^6\,\Theta^2} = \mathrm{C^6\,H^{14}\,\Theta^4}.$$

<div style="text-align:center">Oxide of ethylene.   Glycol.   New body.</div>
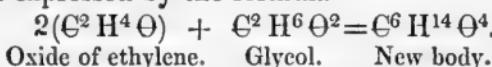
It is also formed, but in very small quantities, by the action of oxide of ethylene on water:

$$3(\mathrm{C^2\,H^4\,\Theta}) + \mathrm{H^2\,\Theta} = \mathrm{C^6\,H^{14}\,\Theta^4}.$$

<div style="text-align:center">Oxide of ethylene.   New body.</div>
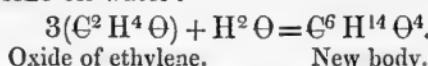
Hence one, two, or three atoms of oxide of ethylene can unite with one atom of water to form, by direct synthesis, more and more complicated bodies, which are nevertheless very simple in their molecular constitution. Wurtz considers these bodies to be alcohols. If the name *ethylenic alcohol* be given to glycol, the other two bodies may be named *diethylenic alcohol* and *triethylenic alcohol.* The following formulæ indicate the relations of these bodies to each other:—

$$\underset{\text{Glycol.}}{\mathrm{C^2\,H^6\,\Theta^2}} = \underset{\text{Ethylenic alcohol.}}{\left.\begin{array}{l}\mathrm{C^2\,H^{4\prime\prime}}\\ \mathrm{H^2}\end{array}\right\}\Theta^2} \quad \begin{array}{c}\text{derivative of the}\\ \text{diatomic type}\end{array} \quad \left.\begin{array}{l}\mathrm{H^2}\\ \mathrm{H^2}\end{array}\right\}\Theta^2.$$

$$\underset{\substack{\text{M. Lourenço's}\\ \text{compound.}}}{\mathrm{C^4\,H^{10}\,\Theta^3}} = \underset{\substack{\text{Diethylenic}\\ \text{alcohol.}}}{\left.\begin{array}{l}\mathrm{C^2\,H^{4\prime\prime}}\\ \mathrm{C^2\,H^4}\\ \mathrm{H^2}\end{array}\right\}\Theta^3} \quad \begin{array}{c}\text{derivative of the}\\ \text{triatomic type}\end{array} \quad \left.\begin{array}{l}\mathrm{H^2}\\ \mathrm{H^2}\\ \mathrm{H^2}\end{array}\right\}\Theta^3.$$

$$\underset{\text{New body.}}{\mathrm{C^6\,H^{14}\,\Theta^4}} = \underset{\text{Triethylenic alcohol.}}{\left.\begin{array}{l}\mathrm{C^2\,H^{4\prime\prime}}\\ \mathrm{C^2\,H^{4\prime\prime}}\\ \mathrm{C^2\,H^{4\prime\prime}}\\ \mathrm{H^2}\end{array}\right\}\Theta^4} \quad \begin{array}{c}\text{derivative of the}\\ \text{tetratomic type}\end{array} \quad \left.\begin{array}{l}\mathrm{H^2}\\ \mathrm{H^2}\\ \mathrm{H^2}\\ \mathrm{H^2}\end{array}\right\}\Theta^4.$$

<div style="text-align:center">* *Comptes Rendus,* November 21, 1859.</div>

Wurtz has also * found that oxide of ethylene can combine directly with ammonia to form very powerful organic bases. When oxide of ethylene is added to a concentrated aqueous solution of ammonia, the two bodies combine with great energy; and on evaporating the mixture, a strongly alkaline liquor is obtained. By neutralization with hydrochloric acid and further evaporation, brilliant colourless rhombohedra are obtained which have the formula

$$C^6 H^{15} N \Theta^3, HCl.$$

With bichloride of platinum this body combines to form a double salt which crystallizes in golden yellow laminæ; the composition of this body is expressed by the formula

$$C^6 H^{15} N \Theta^3, HCl, \dot{P}tCl^2.$$
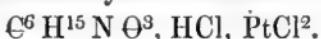
The mother-liquor from the above rhombohedra contains an uncrystallizable hydrochlorate. When this is evaporated and bichloride of platinum added, a double salt is obtained which crystallizes in magnificent orange-red rhomboidal prisms; their composition is

$$C^4 H^{11} N \Theta^2, HCl, PtCl^2.$$

The base contained in the latter platinum-salt contains the elements of an atom of ammonia, and of two atoms of oxide of ethylene; its formation is thus expressed:

$$2C^2 H^4 \Theta + NH^3 = \left.\begin{matrix} C^2 H^4 \Theta \\ C^2 H^4 \Theta \end{matrix}\right\} NH^3 = C^4 H^{11} N\Theta^2.$$
Oxide of ethylene.   New base.
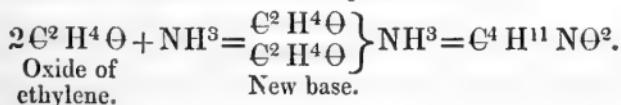
The base contained in the rhombohedric hydrochlorate contains the elements of an atom of ammonia, and of three atoms of oxide of ethylene. Thus:

$$3C^2 H^4 \Theta + NH^3 = \left.\begin{matrix} C^2 H^4 \Theta \\ C^2 H^4 \Theta \\ C^2 H^4 \Theta \end{matrix}\right\} NH^3 = C^6 H^{15} N\Theta^3.$$
Oxide of ethylene.   New base.
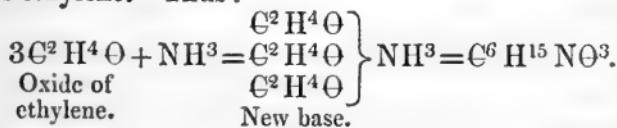
These bases result from the tendency which oxide of ethylene exhibits when added to the elements of another body to form direct combinations, and also to double or triple its molecule. Unlike the compound ammonias, these bases are not formed by substitution, but by direct addition; they are rather to be regarded as conjugated ammonias, and seem to support the idea of Berzelius, that the alkaloids contain ammonia ready formed. Wurtz considers, nevertheless, that they belong to the ammonia type, and reserves for a future communication the discussion of their constitution.

* *Comptes Rendus,* December 3, 1859.

It is obvious, as Wurtz remarks, that by treating oxide of ethylene with compound ammonias, a great variety of artificial alkaloids containing oxygen may be obtained.

Bohn* has investigated the optical relations of the tartaric acid prepared artificially by Liebig, by the action of nitric acid on sugar of milk. He finds that they are quite identical with those of the ordinary tartaric acid.

XVI. *On the Interference of Heat.*
By Professor KNOBLAUCH†.

THE differences of phase observed by Prof. Knoblauch in the ætherial oscillations of interfering thermic rays were produced in the following four different ways.

1. *Difference of phase in consequence of unequal lengths of path in one and the same medium.*

A beam of solar light, after being reflected by a heliostat, entered through a slit, from 4 to 6 millims. wide, into a dark room, and at about 2·3 metres from the window fell upon a glass grate behind which was placed an achromatic glass lens. When a square thermo-electric pile (whose anterior opening could be narrowed or widened by means of wings) was placed at about 0·5 metre from the lens in different parts of the interference-spectrum there formed, a multiplier connected with the thermo-electric pile showed deflections varying, according to the fineness of the grate, from $2°·15$ to $18°·5$ when the pile entered the central white luminous field. The needle of the multiplier returned to its zero-point when the thermoscope was placed in one of the dark bands to the right or left of the centre. It became deflected again, however, to $0°·6$ or $0°·7$ as soon as the instrument was moved into either of the next following coloured spectra. With very fine grates, the increased cold between the first and second spectra could be detected with certainty.

The phænomenon was clearest with finely scratched plates of rock-crystal, behind which the indications of the thermo-multiplier were the following :—

$2°$ in the central white	.	2·5 millims. broad,
$0°$ in the first dark band	.	9·0 millims. broad,
$1°·25$ in the first spectrum	.	8·5 millims. broad,
$0°$ in the second dark band	.	1·25 millims. broad,
$0°·87$ in the second spectrum	.	15·0 millims. broad.

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* *Comptes Rendus,* December 3, 1859.

† From the Monthly Reports of the Royal Academy of Sciences at Berlin.

, In order to diminish to the utmost the absorption in the traversed media, and thus to increase the intensity of the effects, a grate and lens of rock-salt were employed. With such a grate, containing 600 lines in an inch, the deflection for the centre was $31°·0$, and for the first spectrum $1°·5$; these two luminous bands were separated by a colder one corresponding to a deflection of $0°·3$. With a finer grate the deflection was observed to be $17°·25$ in the centre, $3°·5$ in the first spectrum, and only $0°·5$ between the two. The deflections which with rock-salt remain on the dark bands are due to diffusion of the rays, which, with this substance, cannot be avoided.

The above effects can certainly not be ascribed to accidental secondary actions; for the several differences of temperature were still observable when the pile exposed the same surface during its displacements, or even when it presented a greater aperture to the rays in the dark bands.

At the same time a new proof of the divergence of the rays of heat through inflexion may be deduced from these experiments. For whilst, without the grate, the enclosing limits of these rays at the place of measurement were, say, $2·5$ millims. apart, after replacing the grate the extreme limits of heat were not even reached at a distance of 300 millims. on each side of the centre; consequently at places which were 600 millims. distant from each other.

2. *Difference of phase with the same length of path in consequence of the passage of rays through a body of unequal thickness.*

After interference-bands had been produced by introducing into the path of the solar rays an interference-prism in place of the grate, and a cylindrical glass lens in place of the achromatic one or the rock-salt lens, and after the thermo-electric pile had in this case, too, distinguished in the most unmistakeable manner the dark bands from the neighbouring light ones by a deflection of from $0°·25$ to $1°·25$ at the multiplier, a somewhat conical strip of glass was interposed behind the interference-prism, in such a manner that the rays of heat, in order to traverse one-half of the same, had to pass through a greater thickness of glass than was necessary in order to traverse the other half. By this means a displacement of the interference-bands was produced; for the thermo-multiplier indicated a decrease of temperature, on the introduction of the glass, when the thermic pile was at a place of original maximum of heat, and an increase of temperature when the pile was situated in one of the former cold bands: this latter fact is the more significant, since the action was there opposed by the absorption of the interposed glass. With respect to interference, therefore, the influence of unequal thick-

nesses of traversed glass is such that places of greater heat become colder, and *vice versâ.*

3. *Difference of phase through unequal reflexion.*

If, according to the principle of the representation of Newton's rings, solar rays are reflected from a flint-glass convex at its lower surface, and from a plane glass under the former consisting half of flint- and half of crown-glass; if, further, between the two a liquid be introduced which, like clove-oil, is inferior to flint-glass, but superior to crown-glass in refracting power, the rays will in the one case pass first from a greater to a less refracting substance, and then from a less to a greater; whilst in the other case these rays will pass twice, successively, from a greater to a less refracting medium. The interference-phænomena, which in the first case consist of a series of rings with a dark centre, and in the second of a series with a light centre, being thrown on a screen by means of a lens, and the screen being replaced by a thermo-electric pile, the temperature in the one centre is found to be so low that the needle of the multiplier is only deflected $0°\cdot5$, whilst in the other centre it is so high as to cause a deflection of $3°$. Laurel, aniseed, calamus, and cassia oils deport themselves like clove-oil; whilst with lavender, bergamot, and citron oil, &c., as also with water and air, their indices of refraction being even less than that of crown-glass, both centres have a lower temperature.

When the double plate of flint- and crown-glass is replaced by one of calcareous spar bounded by the ordinary surfaces of cleavage, two groups of interference-phænomena are also obtained by employing the first-mentioned oils, since their indices of refraction lie between those of the ordinary and extraordinary rays in the calcareous spar; these phænomena can only be separated, however, by interposing a Nicol's prism between the pile and the interference-apparatus. In the one case, corresponding to the dark centre, a deflection of $0°\cdot25$ was obtained; in the other case, corresponding to the light centre, a deflection of $2°\cdot5$, and these according as the principal section of the Nicol's prism and that of the rhomb of calcareous spar were inclined at $90°$ or were parallel to each other. By every position of the Nicol's prism the centre of the rings had the same low temperature when, between the convex flint-glass and the calcareous spar, one of those substances were interposed whose index of refraction is smaller than that of the extraordinary ray in the calcareous spar.

4. *Difference of phase produced by unequal velocities of doubly-refracted rays.*

In order to obtain rectilinear bands by means of double refraction in the polarizing apparatus, it is best to use two plates of

rock-crystal cut parallel to the natural pyramidal surfaces, to place them one above the other in such a manner that their principal sections form an angle of 90° with each other, and to introduce them between the glass-piece and tourmaline, or between the glass-piece and Nicol. A lens then throws these bands objectively upon a suspended screen or upon the thermo-electric pile.

It appeared to promise interest to examine also in this field the quality of the thermic colours produced by interference. In order to obtain the latter, a thin plate of gypsum was introduced between two Nicol's prisms, 85 millims. in length and 42 millims. in diameter. The test itself was instituted by means of diathermanous substances, such as coloured glasses, placed successively before the pile. Observation proved that equal quantities of rays of heat, after passing through the polarizing apparatus and the gypsum, possess in different degrees the power of traversing the same diathermanous substance according as the principal sections of the polarizing and analysing Nicols cross each other at right angles or are parallel; further, that both these groups of rays differ from that which corresponds to an angle of 45° between the principal sections of the two Nicols, and which constitutes the transition from any thermic colour to its complementary one.

XVII. *Remarks on the behaviour of Mercury as an Electrode. By* T. Strethill Wright, *M.D., President of the Royal Physical Society, Edinburgh*[*].

THE voltaic movements of mercury have been investigated by Davy, Gerbour, Hellwig, Erman, Pfaff, and Runge, and especially by Sir John Herschel, who almost exhausted the subject in the Bakerian Lecture for 1824. The great majority of my observations were similar to those made by the authors above mentioned. I shall therefore not detail them to the Society tonight, but merely bring forward those which appear to be new.

Experiment 1.—An ounce of mercury was poured into a shallow vessel containing a quantity of sulphuric acid diluted with water, sufficient to cover the surface of the metal. The mercury was then connected by a fine copper wire with one of the terminal wires of a galvanometer. The other wire of the galvanometer was armed with a small plate of amalgamated zinc; the whole consequently formed a voltaic circle of zinc, mercury, and dilute acid. On plunging the zinc plate into the acid, the needle was

deflected to 90°, and the mercury, which lay extended against the side of the vessel, contracted itself into a more globular form, and, throwing off the liquid from its surface, appeared above the latter dry and bright. The instant, however, that the mercury had become contracted, the needle of the galvanometer returned to zero; indicating the cessation of the powerful current which had passed at the first completion of the circuit. The zinc plate was then removed from the liquid, and the mercury, after remaining contracted for a short time, reassumed its elongated form and hid itself beneath the acid.

Experiment 2.—The mercury was brought to the contracted state as before, and the zinc plate quickly changed for one of platinum. The fluid metal now quickly extended itself; and at the same time a momentary current of great energy was indicated by the galvanometer, but passing in a contrary direction to that obtained by the former arrangement with zinc.

Experiment 3.—The galvanometer was removed, and the zinc brought for an instant in contact with the mercury; by which means a slight addition of zinc to the mercury was effected. The result was a contracted state of the mercury which continued for eight hours. The globule so contracted was then connected with a plate of platinum immersed in the solution, when an evolution of hydrogen commenced on the surface of the platinum which continued until the mercury suddenly resumed its extended shape.

These experiments appear to indicate that the first effect of communication by a connecting wire between the mercury and zinc, was to set in motion a current which deposited a thin layer or film of hydrogen over the whole surface of the mercury. The metal being thus released from its attraction for the acid, assumed a form still more globular than it possessed in air, in consequence of the support it received from the liquid surrounding it. The extension of the mercury, which occurred after the interruption of the circuit, was probably in consequence of the union of the film of hydrogen with the oxygen of the atmospheric air contained in the water.

Experiment 4.—A portion of mercury A, brought into the contracted state as in Exp. 1, was connected, by means of a thin wire, with a second portion of mercury B. B instantly contracted itself as if A had been a plate of zinc.

Similar but less energetic contractions took place in mercury immersed in solutions of common salt, iodide of potassium, and other alkaline salts.

The rapidity of contraction and extension in the mercury was greatly enhanced by the addition of a little nitric acid to the sulphuric acid solution. Indeed, by placing the extremity of the

connecting wire in such a position that the mercury when extending fell against it, a series of contractions and extensions took place in such rapid succession that the eye had some difficulty in following them.

Experiment 5.—Two portions of mercury were immersed in dilute sulphuric acid; A was brought into connexion with the anode or positive pole of a Daniel's battery of six pairs, and B with the cathode or negative pole of the same. A instantly became covered with a silvery film of oxide, flattened itself out into a thin plate, and slowly crept up to and around B, which was contracted and covered by bubbles of hydrogen.

Experiment 6.—Dilute hydrochloric acid was substituted for the sulphuric acid solution of the last experiment. A instantly became covered with a dark brown coating of chloride, and in this state could be drawn out into long threads or branches. The direction of the current was now suddenly reversed, when A drew in all its branches as by magic, and, after rotating violently, assumed the contracted state.

Experiment 7.—A quantity of mercury was strained through muslin into a solution of common salt in water, and lay in a divided state at the bottom of the vessel. When all was quiet, the induced and interrupted current from the primary wire of a powerful electro-magnetic coil machine was passed through the solution. The globules of mercury instantly began to unite with each other, and did not cease to do so until the whole formed a single mass.

The above experiments seem to me to prove the propositions of Erman :—1st. " That so soon as chemical affinities are excited in the galvanic processes, there takes place at the same time an increased intensity of the attraction of surfaces " (that is, of capillary attraction). 2nd. " That the connexion which has been supposed to exist between capillary or surface attraction and chemical affinity, receives from this a notable confirmation."

It has been already stated that in solutions of chloride of sodium, and other alkaline salts, contraction occurs in mercury when rendered the negative element or cathode of a single circle. When, however, in such solutions mercury is made one of the negative elements of a compound circle of high tension, a stronger chemical affinity is set up between the mercury and the liquid, the metal of the alkaline solution is reduced and combines with the mercury, and the latter yielding to an increase of capillary attraction, instead of contracting, expands.

Experiment 8.—One ounce of mercury was placed in a saucer containing a solution of chloride of sodium, and was connected with the negative wire of a Grove's cell. The positive wire of the same cell, armed with a piece of zinc, was now dipped into the

K 2

solution. The mercury extended itself, and was driven with violence in the direction of the zinc. At the same time the strong currents were set up in the solution which have been so fully described by Herschel and others.

Experiment 9.—A common dinner plate was inverted, and a narrow strip of silver cemented across its bottom. This strip served to retain a large pool of mercury in the centre of the plate. A solution of iodide of potassium was now poured on the plate, and two iron wires dipping into the liquid transmitted the current of twelve small Daniel's cells across the plate and the silver strip. The mass of mercury immediately commenced flowing to and fro between the wires, touching and receiving an impulse from each wire alternately. By employing several pounds of mercury, the last experiment might be rendered a very striking illustration of the galvanoscopic properties of mercury before a large audience.

Undulatory Motions of Mercury.

Two globules of mercury immersed in a solution of chloride of sodium were connected with the ends of the primary wire of an electro-magnetic coil machine. When the machine was slowly set in motion, the mercury undulated in two directions so as to represent a cross. Increasing speed in the action of the break caused the metal to assume successively the form of a star with six, eight, twelve, or more rays. Occasionally, also, these stars would revolve slowly on their centres.

A large pool of mercury under the same circumstances had its entire surface thrown into innumerable waves, which showed, by their mutual interference, figures of remarkable complexity, when examined by a reflected beam of light. These figures were always constant for the same speed of break, power of battery, and shape of the mercury.

Having noticed on several occasions the occurrence of undulatory motions in mercury when traversed by a constant current, I endeavoured to reproduce these motions, but for some time in vain. At last I found that they took place in a solution of chloride of sodium containing a very small quantity of sulphuric acid; in the former of which solutions mercury expands, while in the latter it contracts. This beautiful experiment as perfected was performed as follows :—A plate of zinc three inches in diameter, having a wire soldered to it, is sewed up in muslin and cemented in the centre of a white dinner plate. A quantity of mercury is then poured into the plate until it lies as a fluid ring round the bottom and at about two inches distant from the zinc. The metals are now covered with a clear saturated solution of chloride of sodium. The wire soldered to the central piece of zinc is connected with the positive pole of a platinized zinc

battery* of two cells, while a wire connects the negative pole of the battery with the mercury. The mercurial ring now flattens itself out, and strong currents pass through the solution from the zinc to the mercury. Very dilute sulphuric acid is now added drop by drop, until all at once the currents in the solution stop, and the whole of the inner edge of the mercury is thrown into large waves of equal size which flow rapidly round the circle, the mercury assuming the appearance of a ring with large rounded teeth in rapid rotation. A further addition of acid increases the rapidity of the undulations, and at the same time decreases the size of the waves, until at last it stops them altogether, and the mercury becomes contracted. The mass of the mercury has no rotatory movement even when the undulations are the most energetic. At first the waves often pass in opposite directions in different parts of the ring, until the longer set compels the shorter to change its course, and they all pass round in the same direction.

XVIII. *The Correlation of Physical, Chemical, and Vital Force, and the Conservation of Force in Vital Phænomena. By* Joseph LeConte, *Professor of Geology and Chemistry in the South Carolina College, Columbia*†.

MATTER constantly changes its form, but is itself indestructible, except by the same power which called it into being. The same quantity of matter exists in the universe at all times. So also force changes its form constantly, but is itself indestructible, incapable of increase or diminution, and the same absolute amount of force exists in the universe at all times and for ever. The mutual convertibility of the various forms of force is called " correlation of forces." The invariability of the absolute amount in the midst of constant change is called " conservation of force." This principle of correlation and conservation of force must be looked upon as one of the grandest generalizations in modern science,—a principle startling at first, but when clearly understood and firmly grasped, almost axiomatic. It must be considered a necessary truth, and, as such, is a legitimate basis of deductive reasoning.

The correlation of physical forces is universally recognized as a principle in science, and not only so, but has already been pro-

* I have for some years used this battery, in which the platinum and nitric acid of Grove's battery are changed for the thinnest sheet zinc brushed over with a very dilute solution of chloride of platinum, and a nitro-sulphuric acid consisting of five parts by measure of sulphuric acid to one of nitric acid. Zinc is not acted upon by this mixture.

† From Silliman's American Journal for November 1859.

ductive of many beautiful and useful results; but the correlation of physical and vital forces, while generally recognized as a probable fact, has only been speculated on in a vague and as yet unfruitful manner. The science of life is scarcely yet ripe for the legitimate extension of this principle over its domain. The most elaborate attempt of this kind which I have seen is contained in the very remarkable and suggestive paper of Dr. Carpenter, entitled " Mutual Relation of Physical and Vital Forces," and published in the Philosophical Transactions for the year 1850.

In the present paper I wish simply to present a few thoughts which have originated in my own mind in the course of reflection on this subject, in the hope that they may prove suggestive to others. They have at least the merit of being uninfluenced by the writings of others, and therefore perhaps of presenting the subject in a somewhat new light. I sincerely wish I could present the matter in a more definite form; but it is certain that, where a subject is not perfectly understood, the attempt to give our ideas more definiteness also makes them more questionable. We are obliged to be content with a certain vagueness, in the hope that by the use of right methods a clearness will come after. We must gratefully accept the twilight in the hope that it marks the approach of the full light of day.

There are four planes of material existence which may be regarded as being raised one above the other. The *first* and lowest is the plane of elementary existence; the *second,* the plane of chemical compounds, or mineral kingdom; *third,* the plane of vegetable existence; and *fourth,* of animal existence. Now it is apparently impossible for any known force in nature to raise matter through all these grades at once. On the contrary, there is a special force adapted for the elevation of matter from each plane to the plane above. It is the special function of chemical affinity to raise matter from plane No. 1 to No. 2. All the changes, too, which take place upon plane No. 2 by the mutual reactions of bodies situated on that plane, are under the guidance and control of this force. It is the special prerogative of the force of vegetation—of vegetable life—to lift matter from No. 2 to No. 3, *i. e.* from the condition of mineral matter to the higher condition of vegetable matter. All the changes which take place upon this plane, the laws of which constitute vegetable physiology, are under the guidance of this force. Finally, the force of animal life, and that alone, enjoys the privilege of lifting matter still higher, into the fourth plane, *i. e.* the plane of animal existence. No force in nature can lift from No. 1 to No. 3, or from No. 2 to No. 4. Plants cannot feed entirely upon elementary matter, nor can animals feed upon mineral matter. The reason of this will be seen in the sequel. Thus it seems that

after matter is raised from the elementary to the mineral condition, it requires an additional force of another and peculiar kind to raise it into the vegetable kingdom, and again another accession of force to raise it into the animal kingdom.   These kingdoms are therefore truly represented as successive planes raised one above the other, thus :

> No. 4. Animal kingdom.
>   3. Vegetable kingdom.
>   2. Mineral kingdom.
>   1. Elements.

If, then, it be admitted that this is the relative position of these planes—that it requires a greater and greater expenditure of force to maintain matter upon each successive plane, then it follows *that any amount of matter returning to a lower plane by decomposition must set free or develope a force which may, under favourable circumstances, raise other matter from a lower to a higher condition.*   Or to express it by a mechanical illustration, a given amount of matter falling from one plane to any plane below, developes a force sufficient to raise an equal quantity of matter an equal height.   Thus decomposition must in every case develope force, which force may take the form of heat as in combustion, or electricity as in electrolysis, or may expend itself in forming chemical compounds, or even in organizing matter.

Again, in the same manner as matter may be arranged in several distinct and graduated kingdoms, so it seems to me the forces of nature may also be properly divided into distinct groups arranged in a similar manner one above the other.   These are the physical, the chemical, and the vital forces.   And as in the case of matter, so also in the case of force, it is impossible to pass directly from the lowest to the highest group without passing through the intermediate group.   The conversion of physical into vital force seems impossible without passing through the intermediate condition of chemical force.

These are the simple principles upon which are based all that follows,—principles which may possibly seem fanciful to some unfamiliar with the principle of conservation of force ; but the number of phænomena which they consistently explain will, I hope, entitle them to serious thought.

1st. It is well known that chemical elements, in what is called the "nascent condition," *i. e.* at the moment of liberation from previous combination, exhibit a peculiar energy of chemical affinity not exhibited under other circumstances.   It seems to me that this is readily explicable on the principle of conservation of force.   At the moment of decomposition the chemical affinity which bound the elements together and which was be-

fore satisfied, is suddenly left unsatisfied.   There is an attraction
set free which was before disguised—a force liberated which was
before latent.   If conditions favourable are present, this force may
preserve the form of chemical affinity, and expend itself in form-
ing other chemical compounds, or even, as we shall see here-
after, in organizing matter.   But if favourable conditions are not
present, then it may take some other form of force, *e. g.*, heat or
electricity, and therefore no longer exist as chemical affinity : the
chemical affinity is said to be lost.   To return to the mechanical
illustration used above.   Matter falling from plane No. 2 to
plane No. 1, developes force sufficient to raise other matter from
plane No. 1 to No. 2, but which in the absence of such matter
may expend itself in heat or electricity, or some other form of
physical force.

2nd. It is a fact, now well established, that the seed in germi-
nation forms carbonic acid, and in doing so loses weight : that
is, the organized matter of the seed is partially decomposed, a
portion of its carbon uniting with the oxygen of the air to form
carbonic acid.   Now it is this decomposition which developes the
force by which germination is effected.   A portion of the organic
matter of the seed is decomposed.   This decomposition sets
free a force which suffices to organize the rest.   The force neces-
sary, and therefore the amount of decomposition necessary in this
case is small, because the work to be accomplished is simply the
change from one form of organic matter to another, or rather
from organic to organized matter—to recur again to the former
illustration, merely shifting a certain quantity of matter from
one place to another upon the plane No. 3.   "But how," it may
be asked, "is this decomposition brought about?"   This seems
to be effected by the heat, and perhaps (according to Hunt) by
the actinic rays of the sun[*].   Heat and actinic rays have been
spoken of by many writers, *e. g.* by Carpenter and by Robert
Hunt, as the physical force which is changed into organizing
force by means of the "substratum of an organized structure:"
but the peculiarity of the view which I now present is that this
conversion does not take place immediately, but only *through the
mediation of another force more nearly allied to the vital,* viz. *che-
mical force.*   The food is laid up in the seed mostly in the form
of starch.   In the act of germination this starch is changed into
sugar.   Starch, as is well known, differs from sugar in two im-
portant respects, viz. it is insoluble, and it is more highly car-
bonized[†].   Now, according to the ordinary view, the only object

[*] See Report by Robert Hunt on the Growth of Plants, Rep. Brit.
Assoc. 1846, p. 33 ; 1847, p. 30.

[†] Robert Hunt, Rep. Brit. Assoc. 1847, pp. 20–22. Carpenter, Comp.
Phys. p. 288. Mulder, Physiological Chemistry. pp. 208, 230.

of the partial decomposition is to change the food from an insoluble to a soluble form; and this can be done only by elimination of a portion of the carbon in the form of carbonic acid. According to the view which I now present, *the food is always laid up in a more highly carbonized condition than is wanted, in order that force may be set free by elimination of superfluous carbon.* According to the ordinary view, if an insoluble food could be found capable of conversion into the soluble form without loss of carbon, then germination of the seed might take place without loss of weight, by the direct conversion of heat into vital force. According to my view, *decomposition,* and therefore *loss of weight is absolutely necessary to develope the organizing force,* the loss of weight being in fact the exact measure of that force.

3rd. As soon as the plant developes green leaves, a complete change takes place in its mode of development. It no longer loses weight, but increases in weight. It not only developes, but grows. The reason of this is that the organizing force is no longer developed by decomposition of food laid up within its own tissues, but by the decomposition of food taken *ab externo.* Sunlight is universally admitted to be the physical force concerned in this decomposition. Further, it is generally supposed that there is a direct and immediate conversion of light into vital force in the green leaves of plants. But evidently this is impossible, since the *work done by the light is the separation of the two elements carbon and oxygen.* Light is therefore converted into motion. It is therefore the chemical affinity thus set free which is the force immediately converted into vital force. The food of plants consists of carbonic acid, water, and ammonia (CO^2, HO and NH^3), or in some cases, according to M. Ville, of CO^2, HO and $N*$. Sunlight acting through the medium of the green leaves of plants has the remarkable power of decomposing CO^2. The force thus set free from a latent condition, or the chemical affinity of carbon in a nascent condition, is the force by means of which C, H, O and N are raised to the organic condition†. To return to my former illustration: matter (oxygen) falling from the second to the first plane developes force sufficient to raise other matter from the second to the third plane. Thus it is evidently *impossible, on the principle of conservation of force, that*

* See review of the controversy between Boussingault and Ville on this subject, *Bibl. Univ., Arch. des Sci.* vol. xxx. p. 305. Also Phil. Mag. S. 4. vol. xiii. p. 497. *Ann. des Sci.* S. 4. vol. ii. p. 357. Amer. Journ. Science, vol. xix. p. 409. *Bibl. Univ., Arch. des Sci.* vol. xxviii. p. 335. *Ann. des Sci.* S. 4. vol. vii. p. 5.

† Ammonia is also probably decomposed in the tissues of the leaves of plants (Carpenter, "Correlation of Physical and Vital Forces," Phil. Trans. 1850, p. 732. See also Morren, *Bibl. Univ., Arch. des Sci.*, New Series, vol. v. p. 84). This would of course produce additional organizing force.

plants should feed entirely upon elementary matter; whereas according to the ordinary view of the direct conversion of light into organizing force, there is no reason why plants should not feed entirely on elements, except that one of them, carbon, is insoluble.

4th. There are many other phænomena of vegetable life which receive a ready explanation on this theory. I have said that sunlight has the power of decomposing carbonic acid only in the green leaves of plants. Pale plants, such as the Fungi among cryptogams and the Monotropa among phænogams, have no power to decompose CO_2. These plants, therefore, cannot feed upon chemical compounds—mineral matter. They *must feed upon organic matter*, which organic matter *in its partial decomposition furnishes the force necessary for organization*. If so, then this decomposition, as in the case of germination, must be attended with the elimination of CO_2. Both of these are known to be facts. Pale plants do feed upon organic matter and do evolve CO_2. The necessary connexion of these facts with one another and with the principle of conservation of force, is now for the first time, as far as I know, brought out. The phænomena of nutrition in these plants is similar to that of seeds in germination, except that the latter contain the organic matter already laid up within their own tissues, while the former derive it from decaying vegetable or animal matter taken *ab externo* into their tissues. In this case, too, as in germination, heat is apparently the physical force which effects the decomposition of the organic food, and which is therefore converted indirectly through chemical into vital force. Light is actually unfavourable to this process; for light tends to decompose, not to form CO_2. In both cases therefore the conditions favourable for nutrition are, first, abundance of soluble organic matter, second, absence of light and presence of heat. This is, then, apparently the true reason why germinating plants and pale plants avoid the light. These plants grow by the oxidation of carbon and formation of CO_2. Light decomposes CO_2, and must therefore be antagonistic to its formation, and consequently to the growth of these plants. Whether or not this property of light is entirely limited by the condition of its acting through an organic tissue, is a question yet undetermined. Heat we know is favourable to the oxidation of carbon (combustion, fermentation, putrefaction, &c.) under all circumstances. Has light an opposite property also under all circumstances? or is this opposite property of light limited to the condition of its acting through the medium of an organism? I hope the experiments already commenced, and still in progress, by my brother Prof. John LeConte, and published in the last 'Proceedings' and in the American Journal of Science and Arts,

vol. xxiv. p. 317, will eventually furnish the means of solving this very important problem. I do not wish to anticipate the final results of these experiments; but it seems to me that the negative results thus far obtained rather support the view that the action of light is not thus limited. In all experiments on this subject the light and heat of the sun have been combined. Now heat we know is favourable to combustion. The fact, then, that combined light and heat produced no effect, would seem to indicate that light counteracted the effect of the heat of the sun.

5th. *Etiolated plants,* or plants artificially blanched by exclusion of light, exhibit the same phænomena, and for the same reason. These plants cannot receive their organizing force through the decomposition of CO^2 by sunlight, therefore they are obliged to obtain it from decomposition of organic matter. Hence these plants require organic food ; hence also they evolve CO^2 instead of oxygen. In this case also decomposition of organic matter, with a separation of a portion of the carbon in the form of CO^2, furnishes the organizing force. In the absence of any external organic matter in the form of humus or manure, etiolated plants, like germinating seeds, will feed for awhile upon organic matter previously accumulated in their tissues in the form of starch, and actually lose weight of solid matter[*].

6th. In a most interesting and suggestive article in the *Bibliothèque Universelle (Archives des Sciences*[†]) on the subject of humus, M. Risler shows in the most conclusive manner that organic matter in a soluble condition (soluble humus) is taken up by almost all plants. This fact had been previously proved experimentally by Th. de Saussure ; but having been denied by Liebig, it has been very generally neglected by vegetable physiologists. The doctrine of Liebig and of physiologists generally is that, except in case of pale plants, organic matter is decomposed into CO^2, HO, and NH^3, *i. e.* must fall into the mineral kingdom before it can be absorbed and assimilated by plants, and therefore that organic manures only supply the same substances, and in exactly the same form, which are already supplied, but in insufficient quantities, by the atmosphere. But M. Risler repeats with great care the experiments of De Saussure, and confirms the accuracy of his conclusions. Hyacinths and other bulbs were placed with their roots suspended in water coloured with soluble extract of humus. When these plants were placed in the sun, the water became rapidly decolorized. Other roots, such as carrots, also germinating grains of wheat, were observed to produce the same effects. An extract of humus was exposed at a somewhat elevated temperature to sunlight under a

[*] Carpenter, Comp. Phys. p. 285.
[†] *Bibl. Univ., Arch. des Sci.*, New Series, vol. i. p. 305.

bell-glass. Microscopic plants developed in great abundance. As long as these plants continued to develope, the infusion was transparent and did not putrefy in the slightest degree; and yet there was a constant evolution of CO_2, as shown by analysis of the air in the bell-glass. "Now the cellules formed in the liquid contained carbon. This carbon did not come from the CO_2 of the air, for the liquid, far from absorbing, disengaged CO_2. Therefore the soluble humus must have furnished the carbon directly to the vegetable cells." It could not have furnished it indirectly in the form of CO_2 derived from decomposition of the organic matter, otherwise oxygen, instead of CO_2, would have been eliminated. M. Risler thinks moreover that the embryo in germination takes up soluble organic matter in the form of humus in addition to the soluble organic matter contained within the cotyledons, and that the evolution of CO_2 by germinating seeds is due in part also to the oxidation of humus. Finally, according to the same author, the formation of roots in all plants, but particularly those containing much starch or sugar, is due to the direct absorption of humus, and not, as is generally supposed, to the fixation of carbon by means of light. "In order," says he, "that CO_2 of the air should form these substances, it is necessary, in the beet and the potato, that there should be a descending sap, which there is not." Moreover, if the carbon were taken from the soil in the form of CO_2, there should be elimination of oxygen instead of evolution of CO_2; but the converse is the fact, as has been proved in the most indisputable manner by De Saussure and Boussingault[*].

Mulder is equally explicit in affirming that plants absorb soluble organic matter, which is converted in the roots, by elimination of a portion of the carbon, into starch and sugar (Mulder, pp. 620, 664, 682). Thus, according to these authors, sap is actually elaborated by the roots from organic manures.

Now according to the theory which I propose, this change from humus into starch, sugar, or cellulose, furnishes an additional life-force. Humus is a more highly carbonized substance than either starch or cellulose. By the partial decomposition of humus in the tissues of the plant, with the elimination of a portion of its carbon (removed by oxidation), a chemical force is set free which serves to assimilate the remainder. Hence this process of evolution of CO_2, as we have already said, is opposed by light, but favoured by darkness and heat. Light favours the formation of chlorophyll, of woody fibre, of essential oils, gums, &c.; darkness, heat, and organic manures favour the formation of sugar, starch, &c. Hence the explanation of the well-known fact, that by covering up the lower portions of potato plants by

heaping earth around them, many buds which would otherwise form leafy branches develope into tubers. Hence also the explanation of the equally well-known fact, that the roots of plants seek and grow most rapidly in the direction of most abundant food. If the sap is elaborated entirely in the leaves, it is difficult to understand why the descending sap should flow in greater abundance in one direction than another. But if sap is elaborated in the root itself, it is easy to see why growth is most rapid in the direction of most abundant manure. It is easy to see, too, why roots avoid the light; since light decomposes CO_2, and therefore must be unfavourable to the formation of this substance.

7th. It is a well-known fact that the so-called respiration of plants consists of two distinct and apparently opposite processes: 1st, the absorption of CO_2 by the leaves, and also in solution by the roots, the decomposition of this CO_2 by means of light, with the fixation of the carbon and the elimination of the oxygen; 2nd, the recomposition and evolution of CO_2. The decomposition of CO_2 undoubtedly takes place in the leaves; but where the recomposition of CO_2 takes place is not so well ascertained. It is exhaled, however, like the oxygen, from the leaves. The process of decomposition of CO_2 takes place only during the day, as light is absolutely necessary for this process. The recomposition of CO_2 takes place night and day, although its exhalation, according to some observers, seems to be more abundant during the night. The process of decomposition of CO_2 is well understood; of that of recomposition our knowledge is very imperfect. M. Risler's explanation of this latter process seems most probable. Plants, we have seen, undoubtedly absorb soluble organic matter, *i. e.* humus. Humus we know is a more highly carbonized substance than cellulose or starch. This humus is therefore oxidized in the roots and interior of the trunk, away from light, by means of oxygen, also absorbed by the roots, and thus forms CO_2. This CO_2 then circulates in the sap to be exhaled by the leaves, or perhaps to be again decomposed by sunlight in these organs. In the absence of light the whole is exhaled undecomposed. This readily accounts for the apparently greater exhalation of CO_2 during the night. A series of well-conducted experiments would test the truth of this view. If it is true, there should be a relation between the richness of the soil in organic manures and the amount of CO_2 exhaled. For a given amount of growth, the amount of CO_2 exhaled is the measure of the amount of food taken up in the form of organic matter, and the amount of oxygen exhaled is the measure of the amount of food taken in the form of mineral matter. Or if the exhaled CO_2 is decomposed in the leaves during the day, then of course the difference between the amount exhaled during the night and day

would enter as an element in the calculation. Also, it would seem that those plants especially which frequent rich shady spots, should exhale proportionally more CO_2 and less oxygen, than those loving thin soils and sunny places.

In plants, then, there are two sources of organizing force, the relative proportion of which varies infinitely, according to the amount of light, heat, colour of the plant and richness of the soil in organic matters. The two sources are immediately, 1st, the decomposition of CO_2, 2nd, the decomposition of soluble highly carbonized organic matter; remotely, the two sources are light and heat. In plants which first take possession of desert spots, bare rocks, &c., the first is the only source. In pale plants and fungi the second is the only source; but in most plants the two are combined in various proportions. The first must of course be considered the most fundamental and necessary, the second being evidently supplementary. The decomposition of CO_2 by sunlight may be considered as the original source of all vegetation; but in most of the higher orders of plants the process of nutrition is expedited by the reabsorption of organic matter before it again returns to the condition of CO_2, HO, and NH_3.

8th. The egg during incubation absorbs oxygen, evolves CO_2, and probably HO, and loses weight. As the result of this evolution of CO_2, we find the egg developes. What it loses in weight it gains in organization. Now what is the source of the organizing force? It evidently bears a direct relation to the loss of weight. Here also, then, we have partial decomposition furnishing the necessary force. A portion of the organic matter, falling from the organic to the mineral plane, sets free a force which raises the remaining portion into a slightly higher condition. Heat is evidently the physical force or agent which is transformed, not directly but indirectly, through chemical affinity into vital force; in other words, heat is the agent which effects the necessary decomposition. The phænomenon of development of the egg is therefore very similar to that of the seed.

9th. After the hatching of the egg, the animal no longer loses weight, because recomposition of food taken *ab externo* proceeds *pari passu* with decomposition. But in this case also decomposition supplies the force by which recomposition is effected, and growth and development carried on. As this is an important point, I will attempt to explain it more fully.

It is well known that in the animal body there are, going on constantly, two distinct and apparently opposite processes, viz. decomposition and recomposition of the tissues; and that the energy of the life is exactly in proportion to the rapidity of these processes. Now according to the ordinary view, the animal body must be looked upon as the scene of continual strife between

antagonistic forces, chemical and vital; the former constantly tearing down and destroying, the latter as constantly building up and repairing the breach. In this unnatural warfare the chemical forces are constantly victorious, so that the vital forces are driven to the necessity of contenting themselves with the simple work of reparation. As cell after cell is destroyed by chemical forces, others are put in their place by vital forces, until finally the vital forces give up the unequal contest, and death is the result. I do not know if this view is held by the best scientific minds at the present day as a fact, but it certainly is generally regarded as the most convenient method of representing all the phænomena of animal life, and, as such, has passed into the best literature of the age. Certain it is, however, that the usual belief, even among the best physiologists, is that the animal tissue is in a state of unstable equilibrium; that constant decomposition is the result of this instability; and that this decomposition, and this alone, creates the necessity of recomposition—in other words, creates the necessity of food. But, according to the view which I now propose, decomposition is necessary to develope the force by which organization of food or nutrition is effected, and by which the various purely animal functions of the body are carried on : that decomposition not only creates the necessity, but at the same time furnishes the force of recomposition.

But it will no doubt be objected that, according to the principle of conservation of force, decomposition of a given amount of matter can only effect the recomposition of an equal amount—that a given quantity of matter falling a given height, can only raise an equal quantity an equal height : the whole force developed by decomposition seems to be expended in maintaining the body at a given position. How then can growth and animal activity go on? The answer to this question is obvious enough when we recollect the nature of the food of animals. Animals, it is well known, cannot feed upon mineral matter, but only on food already organized, at least up to the vegetable condition. But when decomposition takes place, the animal matter returns no longer to the vegetable condition from which it was immediately raised, but to the mineral condition. It is decomposed into CO_2 HO and urea. This last substance, though not strictly a mineral substance, is far below the condition of vegetable matter. Thus it is evident that a given quantity of matter falling down from the condition of animal to that of mineral matter, *i. e.* from the 4th to the 2nd plane, would develope force sufficient to lift a larger quantity of matter from the vegetable to the animal condition, *i. e.* from the 3rd to the 4th plane, and yet perhaps leave much residual force unexpended. Thus it is possible, and not

only possible but certain, on the principle of conservation of force, that decomposition of animal tissues should set free a force, a part of which is consumed in the recomposition of a larger amount of new matter and thus maintaining growth, a part in animal heat, and a part in animal activity of all sorts. In this view of the case, we see at once the absolute necessity that the food of animals should be organized. Upon the principle of conservation of force, growth and animal activity—in a word, animal life—would otherwise be impossible.

It follows also from the above, that the higher the organization of the food, the smaller the amount of force necessary to effect assimilation, and therefore the larger the amount of residual force to be expended in animal heat and animal activity. In this we find a ready explanation of the superior activity of carnivorous animals, and the loss of animal activity which results in a state of domestication from the use of vegetable diet; also of the supposed superior activity of men fed upon meat diet.

10th. I have spoken thus far of only one source of vital force in animals, viz. the decomposition of the tissues. I have attempted to show how, upon the principle of conservation of force, this is sufficient to carry on the growth and the activity of the animal organism. But decomposition of the tissues, though the fundamental source—the source characteristic of, and peculiar to animals—of immediate and universal necessity in this kingdom, and in many cases sufficient of itself, is not the only source. There is also in animals, as in plants, a supplemental source, viz. the decomposition of food.

It is well known that the food of animals consists of two kinds, the nitrogenous, such as albumen, fibrine, caseine, &c.; and the non-nitrogenous, such as fat, starch, sugar, gum, &c. According to all physiologists since Liebig, the nitrogenous alone are used in the repair and growth of the tissues. The non-nitrogenous are either quickly consumed in respiration, or else are laid up in the form of fat for future consumption in the same way. Now there can be no doubt that animals may live entirely on nitrogenous food; in which case the whole vital force, whether for assimilation of food or for animal heat and animal activity, is derived from the decomposition of the tissues. This is the case also, apparently, in the starving animal, particularly if lean. But in almost all cases much food in the form of fat, starch, sugar, &c. (non-nitrogenous),[1] is never transformed at all into tissues, but is taken into the blood, gradually decomposed, oxidized in the course of the circulation, changed into CO^2 and HO, and finally removed by exhalation from the lungs. Now what is the object of the non-nitrogenous food, since these do not form any part of the tissues, but are again decomposed and thrown

out of the system? The answer usually given is that such food is used in the animal economy solely as fuel to keep up the animal heat. On this view it is difficult to see why this class of food should be used at all, especially in warm climates. But according to the view which I propose we have here an additional source of vital force. The decomposition of these ternary compounds sets free a force which is used in organizing and assimilating other matter (nitrogenous) and in producing animal activity and animal heat. As in plants, although the decomposition of CO_2 by sunlight is all that is absolutely necessary for growth and development, yet the decomposition of organic food supplies an additional force which greatly increases the vigour and rapidity of vegetation; so in animals, although decomposition of the tissues is all that is absolutely necessary to furnish the force of growth and the phænomena of animal life generally, yet the decomposition of non-nitrogenous organic food furnishes additional force by which growth and animal activity may be maintained without too great an expenditure of the tissues.

11th. In what then consists the essential difference between animals and plants? There can be no doubt that it consists, generally, in their relations to one another and to the mineral kingdom. Plants occupy a middle ground between the mineral and animal kingdom—a necessary halting-place for matter in its upward struggles. But when we attempt to define this relation more accurately, the problem becomes much more difficult. It is indeed probable that no single distinction will be found free from objection. The commonly received and, to a certain extent, very correct idea, is that the essential distinction consists in their relation to CO_2. Plants decompose, and animals recompose CO_2. The beautiful manner in which the two kingdoms stand related to each other through these converse processes is familiar to all. But it is well known that most plants carry on both of these processes at the same time; while some, as fungi, pale plants, &c., only recompose CO_2, like animals. It seems to me that at least an equally good fundamental distinction may be found in this: that in plants the fundamental and necessary source of vital force is the decomposition of its mineral food; while in animals the fundamental source of vital force is the decomposition of its tissues. It is true that in what I have called the supplementary source of vital force they seem to meet on common ground, viz. the decomposition of organic food; but even here there is this essential difference, that in plants this decomposition of organic food is only partial, and therefore furnishes not only force but material for organization; while in animals the decomposition is complete and therefore furnishes only force.

As a necessary result of the above, it would seem that the

" vortex " of Cuvier is characteristic of animals.   There seems no
reason to believe that a tissue once formed in plants is ever de-
composed and regenerated, as is the case in animals.   When
plant-cells decompose, the tissue dies.   Hence the absolute ne-
cessity of continuous growth in plants.   In this kingdom, life is
synonymous with growth.   There is no possibility of life without
growth.   There is no such thing as determinate size, shape, or
duration.   There is no such thing as maturity ; or if so, death
takes place at the same instant.   As cell-life is necessarily of
short duration, and as there is no regeneration of tissues in
plants, it is evident that the life of the tissues must be equally
short.   Thus plant-life can only be maintained by the continual
formation of new tissues and a constant travelling of the vital
force from the old to the new.   In exogenous plants the direc-
tion of travel is from the interior to the exterior ; in endogens
from exterior to interior, and still more from below upwards, by
the continual addition of new matter at the apex.   In fungi, where
there is no such superposition of new tissue upon the old, where
growth takes place by multiplication of cells throughout the
whole plant—in other words, a true interstitial growth as in
animals—since there is no regeneration of tissues, the duration
of the life of the plant is limited by the duration of cell-life.

The respiration of animals, also, differs essentially from that of
plants.   At one time the absorption of CO^2 and exhalation of
O was called the respiration of plants.   It is universally admit-
ted now, however, that this is rather a process of assimilation
than of respiration.   The recomposition and exhalation of CO^2,
as soon as discovered, was very naturally likened to animal re-
spiration, and is in fact looked upon by many, as for example
the physiologist Carpenter, as a true respiration.   But there is
an essential difference between this and animal respiration, which
I have already pointed out.   Its very significance is radically
different.   The essential object of animal respiration is the re-
moval of poisonous decomposed matters from the organism.
The so-called respiration of plants, on the contrary, is rather a
process of assimilation, since by it the too highly carbonized or-
ganic food, by the elimination of a portion of its carbon, is
brought into a proper condition for organization.   A true respi-
ration is necessarily connected with a change of the matter of
the tissues—with the vortex of Cuvier—which has never been
shown to exist in plants.   It is true the exhalation of CO^2 has
been looked upon by some physiologists as indicative of a re-
generation of tissues ; but I have already shown that this is
probably not the case, but, on the contrary, that the CO^2 is
formed by the partial decomposition of highly carbonized or-
ganic food.

12th. The most natural condition of matter is evidently that of

chemical compounds, *i. e.* the mineral kingdom. Matter separated from force would exist, of course, only as elementary matter or on the first plane; but united with force, it is thereby raised into the second plane and continues to exist most naturally there. The third plane is supplied from the second, and the fourth from the third. Thus it is evident that the quantity of matter is greatest on the second and least on the fourth plane. Thus nature may be likened to a pyramid, of which the mineral kingdom forms the base and the animal kingdom the apex. The absolute necessity of this arrangement on the principle of the conservation of force may be thus expressed. Matter, force, and energy are related to one another in physical and organic science somewhat in the same manner as matter, velocity, and momentum in mechanics. The whole energy remaining constant, the greater the intensity of the force (the elevation in the scale of existence) the less the quantity of matter. Thus necessarily results what I have called the pyramid of nature, upon which organic forces work upwards and physical and chemical forces downwards.

13th. As the matter of organisms is not created by them, but is only so much matter withdrawn, borrowed as it were, from the common fund of matter, to be restored at death; so also organic forces cannot be created by organisms, but must be regarded as so much force abstracted from the common fund of force, to be again restored, the whole of it, at death*. If then vital force is only transformed physical force, is it not possible, it will be asked, that physical forces may generate organisms *de novo*? Do not the views presented above support the doctrines of "equivocal generation" and of the original creation of species by physical forces? I answer that the question of the origination of species is left exactly where it was found and where it must always remain, viz. utterly beyond the limits of human science. But although we can never hope by the light of science to know how organisms originated, still all that we do know of the laws of the organic and inorganic world seem to negative the idea that physical or chemical forces acting upon inorganic matter can produce them. Vital force is transformed physical force: true, but the necessary medium of this transformation is an organized fabric; the necessary condition of the existence of vital force is therefore the previous existence of an organism. As the existence of physical forces cannot even be conceived without the previous existence of matter as its necessary substratum, so the existence of vital force is inconceivable without the previous existence of an organized structure as its necessary substratum. In the words of Dr. Carpenter, "It is the speciality of the material substratum thus furnishing the medium or instrument of the metamorphosis which establishes and must ever

* Carpenter, Phil. Trans. 1850, p. 755.

maintain a well-marked boundary line between physical and vital forces. Starting with the abstract notion of force as emanating at once from the Divine will, we might say that this force operating through inorganic matter, manifests itself as electricity, magnetism, light, heat, chemical affinity and mechanical motion; but that when directed through organized structures, it effects the operations of growth, development, and chemico-vital transformations."

XIX. *Notices respecting New Books.*

Elementary Geometrical Drawing. By S. H. WINTER.
London: Longman, Green, Longman and Roberts. 1859.

THIS little handbook belongs to a class a great demand for which has been created by the recently adopted system of competitive examinations in connexion with our civil and military services. These examinations, intended originally as *tests*, have very naturally become *objects* of education, and, as such, liable to great abuse. In general the demand of which we speak simply expresses a desire to pass these examinations with the least possible amount of trouble, and not unfrequently it indicates a tendency to evade the true spirit of the test. Many of the books written to satisfy this demand, professing to help the candidate through his examination rather than to impart to him a thorough knowledge of his subject, are at once inaccurate and superficial—in short, purely injurious. On this account, rather than for the merits of the best of them, they require to be watched and carefully sifted. Convinced of the pernicious effects of such books, we felt compelled, not long ago, to condemn severely a certain treatise on Practical Geometry; today we are glad to be able to speak more favourably of a more modest, and at the same time more genuine work on the same subject. It is evident that Mr. Winter could, if required, write a book of a much higher order, and on this account he has been able to accomplish his simpler task creditably. Few of his readers will fail to learn from him how to construct the more essential geometrical figures; and the more intelligent amongst them, instead of finding their reasoning faculties unexercised, or perhaps unrecognized, are continually invited to seek for a reason for the construction they are taught to make. This is as it ought to be; and we trust that Mr. Winter's little book will meet with the success it deserves.

XX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 75.]

May 26, 1859.—Sir Benjamin C. Brodie, Bart., Pres., in the Chair.

THE following communication was read:—
    " Remarks on Colour-Blindness." By Sir John F. W. Herschel, Bart., F.R.S.
    [Extracted from a Report by Sir J. F. W. Herschel on Mr. Pole's

paper on the same subject*, and communicated at the request of the President and Council.]

I consider this paper as in many respects an exceedingly valuable contribution to our knowledge of the curious subject of colour-blindness—1st, because it is the only clear and consecutive account of that affection which has yet been given by a party affected, in possession of a knowledge of what has yet been said and written on it by others, and of the theories advanced to account for it, and who, from general education and habits of mind, is in a position to discuss his own case scientifically ; and 2ndly, for the reasons the author himself alleges why such a person is really more favourably situated for describing the phenomena of colour-blindness, than any normal-eyed person can possibly be. It is obvious that on the very same principle that the latter considers himself entitled to refer all his perceptions of colour to three primary or elementary sensations— whether these three be red, blue, and yellow, as Mayer (followed in this respect by the generality of those who have written on colours) has done, or red, green, and violet, as suggested by Dr. Young, reasoning on Wollaston's account of the appearance of the spectrum to his eyes—on the very same principle is a person in Mr. Pole's condition, or one of any other description of abnormal colour-vision, quite equally entitled to be heard, when he declares that he refers his sensations of colour to two primary elements, whose combination in various proportions he recognizes, or thinks he recognizes, in all hues presented to him, and which, if he pleases to call yellow and blue, no one can gainsay him ; though, whether these terms express to him the same sensations they suggest to us, or whether his sensation of light with absence of colour corresponds to our white, is a question which must for ever remain open (although I think it probable that such is really the case). All we are entitled to require on receiving such testimony is, that the party giving it should have undergone that sort of *education of the sight and judgment,* especially with reference to the prismatic *decomposition* of natural and artificial colours, for want of which the generality of persons whose vision is unimpeachably normal, appear to entertain very confused notions, and are quite incapable of discussing the subject of colour in a manner satisfactory to the photologist.

It is as necessary to distinguish between our sensations of colour, and the qualities of the light producing them, as it is to distinguish between bitterness, sweetness, sourness, saltness, &c., and the chemical constitution of the several bodies which we call bitter, sweet, &c. Whatever their views of prismatic analysis or composition might suggest to Wollaston and Young, I cannot persuade myself that either of them recognized the *sensation* of greenness as a constituent of the sensations they received in viewing chrome yellow, or the petal of a Marigold on the one hand, and ultramarine, or the blue Salvia on the other ; or that they could fail to recognize a certain redness in the colour of the violet, which Newton appears to have had in view when he regarded the spectrum as a sort of octave of colour, tracing in the repetition of redness in the extreme refrangible

ray, the commencement of a higher octave too feeble to affect the sight in its superior tones. Speaking of my own sensations, I should say that in fresh grass, or the laurel-leaf, I do not recognize the sensation either of blue or of yellow, but something *sui generis*; while, on the other hand, I never fail to be sensible of the presence of the red element in either violet, or any of the hues to which the name of purple is indiscriminately given; and my impression in this respect is borne out by the similar testimony of persons, good judges of colour, whom I have questioned on the subject.

I would wish, then, to be understood as bearing in mind this distinction when speaking of the composition of colours by the super-position of coloured lights on the retina. It seems impossible to reason on the joint or compound sensation which ought to result from the supraposition in the sensorium of any two or more sensations which we may please to call primary; so that if, following common usage, I speak in what follows of red, yellow, and blue (or in reference to Young's theory of red, green, and violet) as *primary colours,* I refer only to the possibility of producing all coloured sensations by the union on the retina of different proportions of lights, competent separately to produce those colours, which is purely a matter of experience.

It is necessary to premise this, when I remark that I by no means regard as a logical sequence Mr. Pole's conclusion in § 15, that because he perceives as colours only yellow and blue, *therefore* the neutral impression resulting from their union must be that sensation which the normal-eyed call green. On the contrary, I am strongly disposed to believe that he sees white as we do, for reasons which I am about to adduce.

Mr. Maxwell has lately announced his inability to form green by the combination of blue and yellow. On the other hand, the pris-matic analysis of the fullest and most vivid yellows (those which excite the sensation of yellowness in the greatest perfection), as the colours of bright yellow flowers, or that of the yellow chromate of mercury, clearly demonstrates the fullness, richness, and brilliancy of their colour to arise from their reflexion of the whole, or nearly the whole of the red, orange, yellow, and green rays, and the sup-pression of all, or nearly all the blue, indigo, and violet portion of the spectrum. On the hypothesis of an analysis of sensation correspond-ing to an analysis of coloured light, these facts would seem incom-patible with the simplicity of the sensation yellow, and it would appear impossible (on that hypothesis) to express them otherwise than by declaring red and green to be primary sensations, and yellow a mixture of them—a proposition which needs only to be understood to be repudiated—quite as decidedly as that the *sensation* of green-ness is a mixture of the *sensations* of blueness and yellowness, and for the same reason; the complete want of suggestion of the so-called simple sensations by the asserted complex ones.

Mr. Maxwell's assertion that blue and yellow do not make green, assuredly appears startling as contradictory to all common experience; but the common experience appealed to is that of artists, dyers, and others in the habit of mixing natural colours as they are presented to

us in pigments, coloured tissues, &c., who have for the most part never seen a prismatic spectrum, or at least attended to its phænomena. The perceptions of colour afforded by such objects are those of white light from which certain rays have been abstracted by absorption, that is to say, they are *negative hues,* or hues of darkness rather than of light, inasmuch as all the colouring of the artist is based, not on the generation, but on the destruction of light. This circumstance, which is not generally recognized, even among educated artists, has vitiated all the language of chromatics as applied to art, and so placed a barrier between the painter and the photologist, which has to be surmounted before they can come to a right understanding of each other's meaning. It is evident, that, to make experiments on the subject free from this objection, absorptive colours must be discarded, at least in bodily mixture with each other. Thus it is true that a dingy green may be produced by rubbing together in powder prussian blue and the yellow chromate of mercury above mentioned; but both these agree in reflecting a considerable, and the latter a very large proportion of green light, to the predominance of which in the joint reflected beam its tint is owing. So also, when blue and yellow liquids (not acting chemically on each other) are mixed, as in water-colour drawings, greens, sometimes very lively ones, are produced. In these cases the yellow absorbs almost all the whole of the incident blue, indigo, and violet light, and the blue a very large proportion of the red, orange, and yellow, both allowing much green to pass; [and to *this,* rather than to a mixture of the other rays, the resulting tint is due.

In the light transmitted by cuprate of ammonia of a certain thickness, the red, orange, yellow, and green are wholly extinguished, while the blue, indigo, and violet are allowed to pass. The result is the fullest and *bluest* blue it is possible to obtain. From this result, compared with that derived from the analysis of natural yellows, it follows that the union on the retina of the yellowest yellow, and the bluest blue, in such proportions that neither shall be in excess, so as to tinge the resulting light either yellow or blue, is *not green, but white.* The same conclusion follows from dividing the spectrum into two, the one portion containing all the less refrangible rays up to the limit of the green and blue, the other all the remaining rays. If the blue portion be suppressed, and the remainder reunited by a refraction in the opposite direction, the resulting beam is *yellow,* if the other, *blue,* both vivid colours—but if neither, *white* of course, and not green, results from the exact recomposition of the original white beam.

It may be objected to this, that in the complementary colours exhibited by doubly-refracted pencils in polarized light, yellow is often found to be complementary to purple, and blue to orange. But in neither of these pairs of colours is the spectrum divided in the manner above indicated; and, moreover, in many instances yellow and blue *are* found as complementary colours in the oppositely polarized pencils; of which examples will be found in the scale of tints produced by sulphate of barytes in my paper "On the Action of Crystallized Bodies in Homogeneous Light" (Phil. Trans. 1820,

Table I.). " Rich yellow" appears also as opposed to " full blue" in the scale of complementary tints exhibited by mica in my " Treatise on Light " (Encyc. Metrop., art. 507). It is not asserted that either a good yellow or a good blue cannot be produced otherwise than in a particular manner, but that they *can* be produced *in* that particular manner, and that *when* so produced, their union affects the eye with no sensation of greenness.

Let two very narrow strips of white paper, A, B, be placed parallel to one another in sunshine, so as to be seen projected on a perfectly black ground (a hollow shadow), and viewed through a prism having the refracting edge parallel to them, the refraction being towards the eye, and let the nearer B be gradually removed towards A, so that the red portions of B's spectrum shall fall upon the green portion of A's. Their union will produce *yellow*, or, if too far advanced, *orange*. On the other hand, it will be seen that the yellow space in B's spectrum on which the blue of A's falls is replaced by a streak of white,—whiteness, and not greenness, being the resultant of the joint action of these rays on the retina. If the strips be made wedge-shaped, tapering to fine points, and A being still white, B be made of paper coloured with the yellow chromate of mercury before mentioned, the whiteness of the streak where the blue of A mixes with the yellow of B near the pointed extremities will be very striking.

There is a certain shade of cobalt-blue glass which insulates, or very nearly so, a definite yellow ray from the rest of the spectrum, suppressing the orange and a great deal of the green. If the spectrum of B, formed and coloured as last described, be viewed through this glass, a very well-defined image of it, clearly separated from its strong red and very faint blue images, will be seen. As the glass in question allows blue rays to pass, the white object, A, besides its definite yellow image, will form a broad blue, indigo, and violet train nearer to the eye. Now let B be gradually brought up towards A, so that the violet, indigo, and blue rays of this train shall coincide in succession with the yellow image of B,—*no sensation of greenness will arise at any part of its movement*. Again, if a white card be laid down on a black surface, the edge nearest the eye, when refracted towards the spectator by a prism, will of course be fringed with the more refrangible half of the spectrum. Let this be viewed through such a glass, and in the blue space so seen introduce one half of a narrow rectangular slip of paper thus coloured, having its upper edge in contact with the lower edge of the white card, the other half projecting laterally beyond the card. In this arrangement the definite image of the yellow paper insulated by the glass will be seen divided into a *yellow* half, projecting beyond the blue fringe, and a purplish- or bluish-white one within it, hardly to be distinguished from the image of the white paper, of which it seems a continuation, and which through the glass in question appears a pale blue. This same purplish tint was observed to arise also under the following circumstances :—Laying down in a good diffused light a paper of an exceedingly beautiful ultramarine blue, and beside it, and somewhat overlapping it, another coloured with the same yellow chromate, I

set upon the line of junction a sheet of glass inclined to the plane of the papers upwards towards the eye, so as to allow the blue to be seen by transmitted light, while the yellow reflected from the glass was at the same time received into the eye. By varying the inclination of the glass, the yellow reflexion could be made more or less vivid, so as either to be nearly imperceptible or quite to *kill* the blue of the paper. But at no stage of its intensity, gradually increased from one to the other extreme, was the slightest tendency to greenness produced. The colour passed from blue to yellow, not through green, but through a pale uncertain purplish tint, not easy to describe, but as remote from green as could be well imagined.

Of course in all such experiments one eye only must be used. Stereoscopic superposition of colour, which at first sight would appear readily available, does not satisfy the requisite conditions, and yields no definite results.

The conclusions from these facts may be summed up as follows :—
1st. That in no case can the sensation of green be produced by the joint action on the eye of two lights, in neither of which, separately, prismatic green exists ; 2ndly. That the joint action of two lights, separately producing the most lively sensations of blue and yellow, does not give rise to that of green, *even when one of them contains in its composition the totality of green light in the spectrum*; and, 3rdly. That all our liveliest sensations of yellow are produced by the joint action of rays, of which those separately exciting the ideas of red and green form a large majority ; and that a decided yellow impression is produced by the union of these only.

From these premises it would seem the easiest possible step to conclude the non-existence of yellow as a primary colour. But this conclusion I am unable to admit in the face of the facts,—1st, that a yellow ray, incapable of prismatic analysis into green and red, may be shown to exist, both in the spectrum and in flames in which soda is present ; and 2ndly, that neither red nor green, as sensations, are in the remotest degree suggested by that yellow in its action on the eye. Whether under these circumstances the vision of normal-eyed persons should be termed trichromic or tetrachromic, seems an open question.

That Mr. Pole's vision is *di*chromic, however, there can be no doubt. If I could ever have entertained any as to the correctness of the views I have embodied of the subject in that epithet, after reading all I have been able to meet with respecting it, this paper would have dispelled it. That he sees blue as we do, there is no ground for doubting ; and I think it extremely likely that his sensation of whiteness is the same as ours. Whether his sensation of yellow corresponds to ours of yellow, or of green, it is impossible to decide, though the former seems to me most likely.

One of the most remarkable of the features of this case, and indeed of all similar ones, is the feebleness of the efficacy of the red rays of the spectrum in point of illuminating power, which certainly very strongly suggests an explanation drawn from the theory of three primary coloured *species of light*, to one of which the colour-blind may be supposed absolutely insensible. Mr. Pole himself evidently

leans to this opinion. I had satisfied myself, however, in the case of the late Mr. Troughton, that the *extreme* red—that pure and definite red which is seen in the solar spectrum only when the more luminous red is suppressed, and in which I cannot persuade myself that any yellow exists, was not invisible to him,—though of course not seen as *red*; and on supplying Mr. Pole with a specimen of a glass, so compounded of a cobalt-blue and a red glass as to transmit positively no vestige of any *other* ray, but *that* copiously, so that a candle seen through it appears considerably luminous, and the window-bars against a cloudy sky are well seen if other light be kept from falling on the eye,—I am informed by him that he saw through it " gas, *fire* and other strong lights with perfect distinctness," and that the colour so seen is a "very deep dark yellow." Now it seems to me impossible to attribute this to any minute per-centage of yellow light of the same refrangibility, which this can be supposed to contain. The purity of its tint is extraordinary ; and its total intensity so small, that supposing it reduced to one-tenth of its illuminating power by the suppression of the whole of its primary red constituent, I cannot imagine that any gas-flame or fire-light would be visible through it, or any other luminous body but the sun.

Still it remains a fact, however explained, that the red rays of the spectrum generally are to the colour-blind comparatively but feebly luminous. Mr. Pole speaks of red in more places than one as " *a darkening power* ;" and in the letter I have received from him in reply to my query as to the visibility of light through the red glass above mentioned, he insists strongly on its action as *darkness*. This, however, can only be understood of the effects of red powders in mixture, and not of red *light* ; and as, to our eyes, an intense blue powder, such as prussian blue, has, besides its colorific effect, a violent darkening one (owing to its feeble luminosity), so, to the colour-blind, red powders, when added to others, contribute but little light in proportion to the bulk they occupy in the mixture, and therefore exercise a darkening power by displacing others more luminous than themselves. I think it therefore very probable that red appears to the colour-blind as yellow-black does to the normal-eyed, or, in other words, that our higher reds are seen by them as we see that shade of brown which verges to yellow—that of the faded leaf of the tulip-tree for instance. Now it is worthy of remark, that it is very difficult for the normal-eyed to become satisfied that the browns are merely *shades* of orange and yellow. *Brownness* (such at least has always been my own impression) is almost as much a distinct sensation as greenness ; so that I am not at all surprised at the expression in § 22, that the " sensation of red as a *dark yellow* is certainly very distinct from *full yellow*," or that a colour-blind person should, after long and careful investigation, arrive at the conclusion that red is not to him a distinct colour. I find all this completely applicable to my own perception of the colour brown.

Mr. Pole (§ 11) appears to lay great stress on the fact, that in a closed colour circle in which red, yellow, and blue are so arranged that each shall graduate into both the others, there occurs in the space where red and blue graduate into each other, "a hue of red

which is to him absolutely insensible," and that this red corresponds *not* to that colour which, under the name of carmine, offers to the normal-eyed the *beau-ideal* of redness, but what they term "crimson." Invisibility, as an element of colour, must not; here be confounded with invisibility as light. It is certain that he *sees* the crimson. It is not to him black, but (just what it ought to be on the supposition that his vision is dichromic, and the union of his colours produces white) a neutral, obscure grey; grey being only an abbreviated expression for feeble illumination by white light. In a circle coloured with three elements graduating into each other, there is no neutral point—none, that is, where whiteness or greyness can exist; but when coloured with only two elements, such as yellow and blue (*positive* yellow and blue, that is, whose union produces white, not green), there are of necessity two neutral points which would be both equally white, *i. e.* equally luminous, if the two extremities of each of the coloured arcs graduated off by similar degrees. But this not being the case with the yellow arc, one of its ends to the colour-blind corresponding to a continuation of the red, and so being deficient in illuminating power, the point of neutrality will be that where a feebler yellow is balanced by a feebler blue, and will therefore be less luminous, *i. e.* less white or more grey than the other neutral spot. It is evident, from the general tenor of Mr. Pole's expressions throughout this paper, that his ideas on the subject of colour are gathered mainly from the study of pigments and absorptive (i. e. *negative*) colours, and not from that of prismatic (or *positive*) ones. In other words, his language is that of the painter, as distinguished from the photologist; the distinction consisting in this—that in the former colour is considered in its contrast with whiteness, in the other with blackness; and thus it is that black is considered by many painters as an element of colour, as whiteness necessarily is by photologists.

I may perhaps be allowed to add a few words as to the statistics of this subject. Dr. Wilson gives it as the result of his inquiries, that one person in every eighteen is colour-blind in some marked degree, and that one in fifty-five confounds red with green. Were the average anything like this, it seems inconceivable that the existence of the phenomenon of colour-blindness, or dichromy, should not be one of vulgar notoriety, or that it should strike almost all uneducated persons, when told of it, as something approaching to absurdity. Nor can I think that in military operations (as, for instance, in the placing of men as sentinels at outposts), the existence, on an average, of one soldier in every fifty-five unable to distinguish a scarlet coat from green grass would not issue in grave inconvenience, and ere this have forced itself into prominence by producing mischief. Among the circle of my own personal acquaintance I have only known two (though, of course, I have heard of and been placed in correspondence with several); and a neighbour of mine, who takes great delight in horticulture, and has a superb collection of exotic flowers, informs me that among the multitude of persons who have seen and admired it, he does not recollect having ever met with one who appeared incapable of appretiating the variety

and richness of the tints, or insensible to the brilliancy of the numerous shades of red and scarlet. It may be, however, that the percentage is on the increase—certainly we *hear* of more cases than formerly ; but this probably arises from the fact of this, like many other subjects, being made more generally matter of conversation.

In further reference to the question of the superposition of colours in the spectrum, or of the intrinsic compositeness of rays of definite refrangibilities, I may mention a phenomenon which I have been led to notice in the prosecution of some experiments on the photographic impressions of the spectrum on papers variously prepared, which appeared to me, *when first noticed*, quite incompatible with the simplicity of those rays at least which occupy the more luminous portion of the spectrum, extending between the lines marked D and E by Fraunhofer, and clearly to demonstrate the presence of green light over nearly the whole of that interval. In these experiments the spectrum formed by two Fraunhofer flint prisms, arranged so as to increase the dispersion, and adjusted to the position of least deviation for the yellow rays, was concentrated by an achromatic lens, and received on the paper placed in its focus, which could be viewed from behind. A series of white papers impregnated with washes of various colourless or very slightly coloured chemical preparations, and dried, were exposed ; and the spectrum being received on them, and the centre of the extreme red image as viewed through a standard glass, adjusted to a fiducial pinhole ; a sensitizing wash of nitrate of silver, or any other fitting preparation, was copiously applied to the exposed surface while under the action of the light. Now, under these circumstances, I uniformly found that whereas the spectrum viewed from behind through the paper exhibited all over the space in question a dazzling very pale straw-yellow, hardly distinguishable from white, yet as the photographic action proceeded, and the translucency of the paper began to be somewhat diminished also by incipient drying, very nearly the whole of that space became occupied by a full and undeniable green colour, so as to give the idea of a distinctly four-coloured spectrum—red, green, blue, and violet ; the yellow being in some instances almost undiscernible, and in others limited to a mere narrow transitional interval rather orange than yellow. It was at the same time evident that a great extinction of light (illumination independent of colour) had also been operated, the vivid glare of the part of the spectrum in question being reduced to a degree of illumination considerably inferior to the red part, or, at all events, not much superior. The change of colour was far greater than could be attributed to any effect of contrast, and was proved decisively not to be due to that cause by hiding the adjacent red and blue when the green remained unaffected in apparent tint.

When, for the photographic preparations wetted as described, ordinary, dry, coloured papers were substituted, the change of colour in question was always produced whenever the thickness of the paper and its absorptive power were not such as to destroy or very much enfeeble the more refrangible light. Taking, as a term of comparison, a purely white, wove, writing-paper, I found that the substitution of writing-paper, tinted with the ordinary cobalt blue com-

monly met with, sufficed to give a very great extension of the green, almost to the extinction of the yellow, while, when the papers used were pale-yellow or clay-coloured, answering to the tints called " buff " or " maize " (nearly approximating to Chevreul's *orangé* 4 and 3), and which might naturally have been expected to transmit yellow rays more abundantly at all events than the blue, the spectra (viewed at the back of the papers) were particularly full and abundant in green, occupying the whole of the debateable ground. In the case of the former, a narrow yellow space was seen, and the blue was very much enfeebled, and separated from the green by a very perceptible suddenness of transition. With the latter the green was finely exhibited, and the yellow confined to a narrow orange-yellow border : the blue and violet much enfeebled.

On further considering these facts, there seemed to be but three ways of accounting for them :—1st, by the effect of contrast. This I consider to be disposed of by the suppression of the adjacent colours, as recorded above. 2ndly, by extinction of a *yellow element* of colour over the space DE, allowing a substratum of green to survive ; or, which comes to the same, by the extinction of the red element over the same space, which, by its combination with (an assumed elementary) green, produced the original brilliant straw-yellow. And 3rdly, by admitting as a principle, that our judgment of colours absolutely, *in se,* and independent of contrast, is influenced by the *intensity* of the light by which they affect the eye, and that very vivid illumination enfeebles or even destroys the perception of colour. As the apparent change of colour from pale-yellow to green in the cases above related was always accompanied with a great diminution of general intensity, it occurred to me to produce such diminution by optical means, which should operate equally on all the coloured rays, and diminish all their intensities in the same ratio. This was accomplished by viewing the spectrum (as projected on purely white paper) by reflexion on black glass, or by two successive reflexions in different planes, and I found the very same effect to take place. That portion DE of the spectrum which in the unreflected state appeared dazzlingly bright and nearly colourless, was seen by one such reflexion, and still more so by two, green. The extension of the green region was greater, and the limitation of the yellow portion more complete, according to the amount of illumination destroyed by varying the angles of incidence on the glasses. When much enfeebled by two cross reflexions, the aspect of the spectrum was that represented in Chevreul's coloured picture of it from the line A to H. When enfeebled by other means, as by viewing the spectrum thrown on a blackened surface, the effect was exactly the same.

The last of our three alternatives, then, would appear to be established as the true explanation ; and in respect of the second, it is eliminated by the consideration that neither the slight degree of coloration in the bluish papers, or the tint of the pale-yellow ones which effected the change, would give rise to so great a *preferential* extinction of yellow or red rays as an explanation founded on that alternative would require. The phenomenon is certainly a very

striking one, and has created great surprise in those to whom I have shown it.

[Continued from p. 78.]

December 14, 1859.—Prof. J. Phillips, President, in the Chair.

The following communications were read :—

1. "On some Remains of *Polyptychodon* from Dorking." By Prof. Owen, F.R.S., F.G.S.

Referring to the genus of Saurians which he had founded in 1841 on certain large detached teeth from the Cretaceous beds of Kent and Sussex, and which genus, in reference to the many-ridged or folded character of the enamel of those teeth, he had proposed to call *Polyptychodon*, Prof. Owen noticed the successive discoveries of portions of jaws, one showing the thecodont implantation of those teeth, which, with the shape and proportions of the teeth, led him to suspect the crocodilian affinities of *Polyptychodon*; and the subsequent discovery of bones in a Lower Greensand quarry at Hythe, which, on the hypothesis of their having belonged to *Polyptychodon*, had led him to suspect that the genus conformed to the Plesiosauroid type.

The fossils now exhibited by Mr. G. Cubitt of Denbies, consisted of part of the cranium (showing a large foramen parietale), fragments of the upper and lower jaws, and teeth of the *Polyptychodon interruptus*, from the Lower Chalk of Dorking, and afforded further evidence of the plesiosauroid affinities of the genus. Professor Owen remarked that in a collection of fossils from the Upper Greensand near Cambridge, now in the Woodwardian Museum, and in another collection of fossils from the Greensand beds near Kursk in Russia, submitted to the Professor's examination by Col. Kiprianoff, there are teeth of *Polyptychodon*, associated with plesiosauroid vertebræ of the same proportional magnitude, and with portions of large limb-bones, without medullary cavity, and of plesiosauroid shape.

Thus the evidence at present obtained respecting this huge, but hitherto problematical, carnivorous Saurian of the Cretaceous period seemed to prove it to be a marine one, more closely adhering to the prevailing type of the Sea-lizards of the great mesozoic epoch, then drawing to its close, than to the *Mosasaurus* of the Upper Chalk, which, by its vertebral, palatal, and dental characters, seemed to foreshadow the Saurian type to follow.

Prof. Owen exhibited also drawings of specimens in the Woodwardian Museum and in the Collection of Mr. W. Harris, of Charing, which show the mode and degree of use or abrasion to which the teeth of *Polyptychodon* had been subject.

2. "On some Fossils from near Bahia, South America." By S. Allport, Esq.

The south-west point of the hill on which the Fort of Montserrate is built, in Bahia Bay, exhibits a section of alternating beds of conglomerate, sandstone, and shale; in the last Mr. Allport discovered

a large Dinosaurian dorsal vertebra, not unlike that of *Megalosaurus,* several Crocodilian teeth, and numerous large scales of *Lepidotus,* together with a few Molluscs (*Paludina, Unio,* &c.), some *Entomostraca,* and Lignite. Two miles from Montserrate, in a N.E. direction, is the Plantaforma, another hill of the same formation, but loftier. The shales here also yielded similar fossils.

These fossiliferous shales and conglomerates dip to the N.W. towards the Bay, and appear to overlie a similarly inclined whitish sandstone, which rests against the gneissose hills ranging north-eastwardly from the point of St. Antonio.

3. "On a Terrestrial Mollusc, a Chilognathous Myriapod, and some new species of Reptiles, from the Coal-formation of Nova Scotia." By J. W. Dawson, LL.D., F.G.S. &c.

On revisiting the South Joggins in the past summer, Dr. Dawson had the opportunity of examining the interior of another erect tree in the same bed which had afforded the fossil stump from which the remains of *Dendrerpeton Acadianum* and other terrestrial animals were obtained in 1851 by Sir C. Lyell and himself. This second trunk was about 15 inches in diameter, and was much more richly stored with animal remains than that previously met with. There were here numerous specimens of the land-shell found in the tree previously discovered in this bed ; several individuals of an articulated animal, probably a Myriapod ; portions of two skeletons of *Dendrerpeton* ; and seven small skeletons belonging to another Reptilian genus, and probably to three species.

The bottom of the trunk was floored with a thin layer of carbonized bark. On this was a bed of fragments of mineral charcoal (having Sigillarioid cell-structure), an inch thick, with a few Reptilian bones and a *Sternbergia*-cast. Above this, the trunk was occupied, to a height of about 6 inches, with a hard black laminated material, consisting of fine sand and carbonized vegetable matter, cemented by carbonate of lime. In this occurred most of the animal remains, with coprolites, and with leaves of *Noeggerathia* (*Poacites*), *Carpolithes,* and *Calamites,* also many small pieces of mineral charcoal showing the structures of *Lepidodendron, Stigmaria,* and the leaf-stalks of Ferns. The upper part of this carbonaceous mass alternated with fine grey sandstone, which filled the remainder of the trunk as far as seen. The author remarked that this tree, like other erect *Sigillariæ* in this section, became hollow by decay, after having been more or less buried in sediment ; but that, unlike most others, it remained hollow for some time in the soil of a forest, receiving small quantities of earthy and vegetable matter, falling into it, or washed in by rains. In this state it was probably a place of residence for the snails and myriapods and a trap and tomb for the reptiles ; though the presence of coprolitic matter would seem to show that, in some instances at least, the latter could exist for a time in their underground prison. The occurrence of so many skeletons, with a hundred or more specimens of land-snails and myriapods, in a cylinder only 15 inches in diameter proves that these creatures were by no means rare in the coal-forests ; and the conditions of the tree with its air-

breathing inhabitants implies that the Sigillarian forests were not so low and wet as we are apt to imagine.

The little land-shell, specimens of which with the mouth entire have now occurred to the author, is named by him *Pupa vetusta.* Dr. Dawson found entire shells of *Physa heterostropha* in the stomach of *Menobranchus lateralis,* and hence he supposes that the *Pupæ* may have been the food of the little reptiles the remains of which are associated with them.

Two examples of *Spirorbis carbonarius* also occurred ; these may have been drifted into the hollow trunk whilst they were adherent to vegetable fragments. The Myriapod is named *Xylobius Sigillariæ,* and is regarded as being allied to *Iulus.*

The reptilian bones, scutes, and teeth referable to *Dendrerpeton Acadianum* bear out the supposition of its Labyrinthodont affinities. Those of the new genus, *Hylonomus,* established by Dr. Dawson on the other reptilian remains, indicate a type remote from *Archegosaurus* and *Labyrinthodon,* but in many respects approaching the Lacertians. The three species determined by the author are named *H. Lyellii, H. aciedentatus,* and *H. Wymani.*

4. " On the Occurrence of Footsteps of *Chirotherium* in the Upper Keuper of Warwickshire." By the Rev. P. B. Brodie, F.G.S.

True Chirotherian footsteps do not appear to have been hitherto met with in the Keuper of Warwickshire ; but a specimen of Keuper sandstone showing the casts of a fore and a hind foot of *Chirotherium* was lately turned up by the plough at Whitley Green near Henley-in-Arden. The breadth of the fore foot is about 2 inches ; the hind foot is $4\frac{1}{2}$ inches across. As the New Red sandstone of Cheshire, so well known for its fine Chirotherian foot-tracks, certainly belongs to the upper part of the New Red series, it may now be further correlated with the Upper Keuper of Warwickshire, the latter having yielded true Chirotherian foot-prints.

January 4, 1860.—Prof. J. Phillips, President, in the Chair.

The following communications were read :—

1. " On the Flora of the Silurian, Devonian, and Lower Carboniferous Formations." By Prof. H. R. Goeppert, For. Mem. G. S.

The number of all the fossil plants which the author has described as belonging to these formations (chiefly from Germany) amounts to 184 species: Algæ, 30 species; Calamineæ, 20 ; Asterophylliteæ, 4 ; Filices, 64 ; Selagineæ, 39 ; Cladoxyleæ, 4 ; Noeggerathiæ, 8 ; Sigillariæ, 6 ; Coniferæ, 6 ; Fruits (uncertain), 3.

Prof. Goeppert has seen only *Algæ* from the Silurian Rocks. *Sigillaria Hausmanni* is one of the most interesting of the Lower Devonian plants, and *Sagenaria Weltheimiana* of the Middle Devonian. The Upper Devonian has several terrestrial plants. Of the Lower Carboniferous Flora, the following are the most important and characteristic plants :—*Calamites Transitionis, C. Roemeri,* and *Sagenaria Weltheimiana.* The last name supersedes *Knorria imbricata.*

2. " On the Freshwater Deposits of Bessarabia, Moldavia, Wallachia, and Bulgaria." By Capt. T. Spratt, R.N., C.B., F.R.S., F.G.S.

Capt. Spratt first referred to the many isolated patches of fresh-

water deposits in the Grecian Archipelago and in the neighbouring countries, also around the Black Sea, to which others have alluded, or which have been described by himself as evidences of the existence of a great freshwater lake, probably of middle tertiary age.

On the borders of the Yalpuk Lake, in Southern Bessarabia, are sections exhibiting old lacustrine deposits containing similar fossils to those found elsewhere by Capt. Spratt in the strata referred by him to the extensive oriental lake of the middle tertiary period. Among these fossils are freshwater Cockles ; such as are associated with *Dreissena polymorpha* in the strata at the Dardanelles and elsewhere. After some search, Capt. Spratt found similar Cockles living in the Yalpuk lake ; and from this evidence, and from the relatively different levels of the old lacustrine deposits and the present Black Sea, he satisfied himself of the really freshwater condition of the old tertiary lake,—the Black Sea area having been separated from the old lacustrine area of Bessarabia and the Provinces by a barrier at the Isaktcha hills which the Danube has since cut through. Capt. Spratt remarked that the lacustrine conditions of the great area in Eastern Europe and Asia Minor where he has indicated freshwater deposits were probably interfered with by volcanic outbursts, which opened a communication between the Euxine and Mediterranean, altering the levels of the region, causing the formation of the great gravel-beds at the foot of the Carpathians, and an outspreading of the brown marly superficial deposits of the Steppe, which are locally impregnated with mineral or marine salts, indicative either of the influx of the sea, or of mineral solutions set free by volcanic agencies.

Capt. Spratt also described the older rocks, some probably of Triassic age, and others Cretaceous, which are here conformably overlain by the lacustrine deposits. These he saw in the hills, south of the Danube, near Tultcha and Beshtepeh ; also at the Raselm Lagoon, where Cretaceous shales and marble containing *Ceratites*, &c. occur ; the latter at Popin Island. At Dolashina, a cape south of the Raselm Lagoon, the soft Cretaceous limestone is full of small *Inocerami*.

These indications of Secondary rocks are intimately connected with those further south, at Cape Media and Kanara, formerly described by the author.

3. " On the Recent and Fossil *Foraminifera* of the Mediterranean Area." By T. Rupert Jones, F.G.S., and W. K. Parker, Mem. Micr. Soc.

The authors presented an extensive Table of the Species and varieties of recent *Foraminifera* from several localities in the Mediterranean (worked out from material gathered and dredged by Capt. Spratt, Mr. Hamilton, Prof. Meneghini, and other friends), and of the fossil forms from the Tertiary deposits of Malaga (Spain), Turin, Sienna, Palermo, and Malta (communicated by Prof. Ansted, Prof. Meneghini, and the Marchese C. Strozzi, or supplied from the Society's Museum) ; also the fossil *Foraminifera* from Baljik supplied by Capt. Spratt, and those of the Vienna Basin as elaborated by D'Orbigny, Czjeck, and Reuss. The recent *Foraminifera*, tabulated

in eleven columns, were illustrative of the range of the respective species and varieties in different zones of sea-depth, from the shore to 1700 fathoms, and of the relative size of the individuals, and of their proportional paucity or abundance. Among the seventeen columns of fossil *Foraminifera*, some were very rich in species and varieties, especially in the case of the Siennese clays, the Malaga clay, and the Vienna basin. From the evidence afforded by the comparison of the fossil with the recent *Foraminifera*, the Siennese blue clays of S. Cerajolo, S. Donnino, S. Lazaro, and Coroncino were regarded as having been deposited in various depths of from 40 to 100 fathoms; so also the clay-beds of Malaga and of the Vienna basin. A blue clay from S. Quirico was probably formed in about 200 fathoms; a blue clay from Pescajo, on the contrary, was the deposit of a shallow estuary. A sand from Pienza, and others from Montipoli, Castel' Arquato, and San Frediano, contain *Amphistegina*, and were probably deposited in from 10 to 20 fathoms water. As the *Amphistegina* appears now to be extinct as regards the Mediterranean, these Amphistegina-beds, and others at Palermo and in the Vienna Basin, may be of miocene age. Another Siennese clay from Monti Arioso is of shallow-water formation. From Turin some shelly sands, of pliocene age, were defined as containing a group of *Foraminifera* similar to those now living on the western shores of Italy; and the Palermo deposits are, for the most part, not very dissimilar. The Heterostegina-bed at Malta, formed probably in rather shallow water, is characterized by a species now absent from the Mediterranean. The tertiary deposit from Baljik appears to have been a shallow-water deposit, characterized by some forms peculiar at the present day to the Red Sea,—a condition that is also indicated by some of the Viennese deposits.

XXI. *Intelligence and Miscellaneous Articles.*

OPTICAL LECTURE-EXPERIMENTS. BY PROF. KNOBLAUCH.

DID I not hope to furnish my colleagues with a few welcome experiments for the lecture table, I should scarcely venture to occupy these pages with a communication which involves nothing but what has been long well known.

Optical lenses have been so universally constructed of substances whose indices of refraction exceed that of the surrounding medium (such being alone useful in practice), that, accustomed to the phænomena dependent thereupon, we unconsciously associate convergent effects with convex, and divergent ones with concave lenses.

In order strikingly to illustrate by experiment the influence which here, under the same form of the limiting surfaces, the enclosed medium exerts, I introduce, in my lectures on experimental physics, experiments with hollow lenses so composed of plane glass discs and watch-glasses, that the one forms a plano-convex, the other a plano-concave lens*.

* The radius of curvature here is about 116 millims.; the aperture of the setting has a diameter of about 64 millims.

After filling both lenses with water, the first is used in a dark room to obtain all the phænomena presented by the objective collection, behind the lens, of all the luminous rays which pass through the same : for instance, the principal focus is determined, and the displacement of the same by the approach of a luminous point is shown ; this displacement is continued until the rays leave the lens in parallel directions, and subsequently the objective focus becomes a virtual one. When the luminous point is replaced by a luminous object, the convex water-lens also presents the series of diminished, equal, and magnified inverted images, and at sufficiently small distances of the object, the magnified uninverted virtual images.

The concave water-lens presents in air the simple phænomena of a diverging lens. It serves to demonstrate that, for every distance of the luminous point, the rays diverge on leaving the lens, that the foci are in consequence all virtual ones, and that in all cases diminished uninverted images are alone observed.

If the media in question are now interchanged, that is to say, if the lenses are filled with air and surrounded by water, the optical phænomena, as may be easily seen, will be all reversed.

The convex lens will become a diverging one with a virtual principal focus, whose distance from the lens may be easily measured (according to Eisenlohr's method) by receiving upon a white screen the divergent solar rays proceeding from the lens in such a manner that the illuminated surface has a diameter double that of the lens. The distance of the screen from the lens will then be equal to the principal focal distance*. All objects (*e. g.* flame of a candle) behind the lens now appear diminished in the same manner as we usually find them to be with a concave glass lens.

The concave air-lens, however, has now under water become a converging lens, whose principal focal length may be found by immediately ascertaining the point of convergence of incident solar rays. By means of a white screen immersed in the water, the course of the rays before and after convergence may be clearly followed, and the change of position of the focus, with changing distances of the luminous point, easily proved†. A piece of ground glass, protected from contact with the water by a second glass, is better than a nontransparent white screen for receiving the objective inverted images : for the screen can only be regarded from above, and the images upon it appear to be raised and distorted ; whilst the ground glass can be looked at from behind, whence the images appear both sharp and un-

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* I employ a similar method in catoptrics in order to measure the principal focal distance of a convex mirror. Thus between the mirror and the sun, and perpendicular to the rays from the latter, is placed a screen in which is a circular hole equal in magnitude to the portion of the mirror required to be operated upon ; around the hole, and concentric with it, is described a circle of double its diameter on the side of the screen facing the mirror. When the screen is so placed that the reflected rays exactly fill this circle, its distance from the mirror is equal to the required distance of the virtual focus.

† If it were required to surround the luminous point with water, the electric light between coal-points might be used.

changed when the eyes are on the same level and look through the transparent sides of the box containing the water*. After illustrating in this manner the changing magnitude of the images, the objective foci may be made to pass into virtual ones, and the corresponding uninverted magnified images will make their appearance.

The agreement between convex lenses of water surrounded by air, and concave lenses of air in water, may be followed further by distinguishing the central and circumferential rays. So long as the foci remain objective, the circumferential rays intersect nearer to the lens than the central ones, but further from it when the foci are virtual. With the concave water-lens surrounded by air, and the convex air-lens in water, the foci are all virtual, and those of the circumferential rays are always nearer to the lens than the foci of the central rays.

Although all this is easily explained by the simple laws of refraction, it was always instructive and surprising to spectators to observe how a mere change of medium converts a diverging lens into a burning glass, and a microscope into a telescope.

To complete the above experiments, the action of a concave lens may be destroyed by a convex one on placing one behind the other in the water; or if, instead of being all equal, the curvatures of the lenses be properly adjusted, a Galilean telescope or a Brücke's magnifier may be constructed with a concave object-glass and a convex eyepiece.

The consequences of the above phænomena of refraction, relative to the phænomena of reflexion which present themselves with water-lenses in air and air-lenses in water, also hold good.

These hollow lenses, too, serve to compare with each other the several effects presented by differently refracting bodies, such as water and clove-oil, with the same form of lens in different media.—Poggendorff's *Annalen*, vol. cvii. p. 323.

ON THE FIXATION OF THE MAGNETIC IMAGE. BY M. J. NICKLÈS.

The name of *magnetic image* is given to the appearance observed when iron filings are placed on a paper screen over the poles of a powerful magnet. It may be fixed in the following manner :—A sheet of waxed paper is placed over the poles of a powerful magnet, and kept in its position by means of a screen interposed between the paper and the poles. The image is then developed in the usual way; and when this is effected, a hot brick or crucible cover is brought near enough to melt the wax. The melted wax by capillarity penetrates the agglomeration of filings, just as water penetrates a mass of sand. It is necessary that the layer of wax have a considerable thickness, in order to be sufficient for the action of capillarity. On cooling, the wax retains the filings in their place, and they present the same appearance as if still under the influence of the magnet.—*Comptes Rendus*, Nov. 27, 1859.

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* This box is 1 metre long, 170 millims. high, and 110 millims. broad.

XXII. *On certain Laws of Chromatic Dispersion.*
By Mungo Ponton, *F.R.S.E.*[*]

THE attempts hitherto made to determine the laws of chromatic dispersion have generally proceeded on the principle of assuming a certain law, and endeavouring, on its basis, to construct a universal formula, which shall render it possible, from having given the refractive indices of two or three of the fixed lines of the spectrum for any medium, to find those of the remainder. These attempts are known to have been attended with but imperfect success.

It has accordingly been deemed expedient to adopt a different course,—to assume the indices of *all* the seven fixed lines for every refracting medium that has been examined, to be, as determined by observation, *nearly* correct; to analyse and compare these indices with a view to discover the hidden law or laws which they involve; and, the general nature of these laws having been thus brought to light, then to make on the observed indices such corrections as may be required to bring them into strict agreement with the laws so ascertained. Thus, without assuming any one or more of the observed indices to be absolutely accurate, as in the former method of proceeding, it may be found possible so to correct each index as to obtain results harmonious in themselves, and all agreeing with determinate general laws. The following is a brief outline of the results to which this method of research has led.

The most general formula under which the law of chromatic dispersion can be expressed is the following. Let U represent

the length of undulation corresponding to a luminous wave that should occupy the place of any one of the fixed lines of the spectrum in the free æther, as determined by Fraunhofer's method of transmitting a divergent beam through a system of fine equidistant lines so as to obtain chromatic dispersion without refraction. Let μ be the refractive index of this undulation in any medium. Call $\dfrac{U}{\mu} = u$ the length of the undulation within the medium after refraction. Then the relation between U and u may be expressed by the general formula $\dfrac{U}{\epsilon} - a \pm x = u$; or conversely, $\epsilon(u + a \pm x) = U$. Here the quantities ϵ and a are *constant* for the same medium and temperature, being the same for all undulations. The quantity x, again, which is comparatively minute, is peculiar to each wave, the medium and temperature remaining the same. It is not, however, a mere irregular fragment applied either to remove an excess or supply a deficiency, but it is subject to symmetrical laws to be hereafter explained. Suffice it meanwhile to state that these variable quantities, represented by x, added to, or subtracted from each wavelength, constitute the *irrationality* of the spectrum.

Save for these small quantities, every spectrum formed by a refracting medium would present the same appearance as the spectrum formed by transmitting a divergent beam through a system of equidistant fine lines. The fixed lines of the spectrum, whatever might be their *actual* distances, would preserve the same *relative* mutual distances. The above formula would thus assume the yet simpler form of $\dfrac{U}{\epsilon} - a = u$, or $\epsilon(u + a) = U$. There would then be no irrationality in the various spectra; and the constant relations of the fixed lines B, C, D, &c. to the same lines when refracted, or to b, c, d, &c., would always be

$$\frac{B-C}{\epsilon} = b - c, \quad \frac{C-D}{\epsilon} = c - d, \quad \frac{B-D}{\epsilon} = b - d,$$

and so on.

In every refracting medium, however, the fixed lines are more or less *extruded* or thrust out of the places which they occupy in the unrefracted spectrum, and their mutual distances are altered; so that the above relation no longer subsists. This extrusion constitutes the irrationality, and every medium accordingly possesses an *extrusive* power besides its *refractive* and *dispersive* powers.

To study the laws by which these several powers are governed, it is needful to separate the effects due to each, and especially, in the first place, to eliminate the quantity x from the formula with

a view to determine the constants ϵ and a. Seeing that, in the absence of x, the difference between the wave-lengths corresponding to any two fixed lines before refraction, when divided by the difference between those same two wave-lengths within the refracting medium, would always be equal to ϵ, it follows that, by taking the *whole* of the fractions which may be thus formed, we shall obtain as many values of ϵ as there are fractions; and by taking the average of all these, the effects produced on the value of ϵ by the variable quantity x will be mutually neutralized.

Hence, by taking the sum of the 21 differences between the wave-lengths corresponding to the seven fixed lines before refraction (or as they exist in the unrefracted spectrum), and dividing them by the sum of their 21 differences within the refracting medium, the quotient will be ϵ. The wave-length within the medium is found by dividing each normal wave-length by its own refractive index; so that, μ being the index, we have $\dfrac{B}{\mu_{,}} = b$, $\dfrac{C}{\mu} = c$, &c. In this manner the positive and negative extrusions are made to neutralize each other. The process, however, may be shortened thus:

$$\frac{(3B + 2C + D) - (F + 2G + 3H)}{(3b + 2c + d) - (f + 2g + 3h)} = \epsilon.$$

The numerator of this fraction is of course constant for all media, and its denominator varies with the refractive indices.

The constant ϵ having been thus found, a may be easily determined as follows. Call $B + C + D + E + F + G + H = S$, which is of course constant. Call $b + c + d + e + f + g + h = s$, which will vary with the indices. Then is $\dfrac{S - \epsilon s}{7\epsilon} = a$, or $\dfrac{\dfrac{S}{\epsilon} - s}{7} = a$.

The two constants ϵ and a having been thus determined, it is easy to find a second series b_2, c_2, d_2, &c., showing what the wave-lengths within the medium would be, apart from their extrusions; that is, if the fixed lines retained the same relative mutual distances within the medium as they present in the unrefracted spectrum. This series is obtained by the formula

$$\frac{B}{\epsilon} - a = b_2, \quad \frac{C}{\epsilon} - a = c_2, \quad \frac{D}{\epsilon} - a = d_2, \text{ &c.}$$

In this series, b_2, c_2, d_2, &c., the following relations constantly subsist: $\epsilon(b_2 - c_2) = B - C$, $\epsilon(c_2 - d_2) = C - D$, and so of the others. Then the differences between b and b_2, between c and c_2, between d and d_2, &c. are the extrusions. In this manner the extrusion due to each of the fixed lines may be ascertained in all those media whose seven indices have been observed with sufficient accuracy;

and this having been accomplished, the mutual relations of these extrusions, whether in the same or in different media, and the laws by which they are governed, may be advantageously studied. This is indeed the most important branch of the inquiry; for unless it can be shown that the extrusions follow some determinate law, the general formula becomes indefinite.

A careful analysis and comparison of the extrusions of the fixed lines in various media have brought to light the following general principles and laws.

The extrusive power of a medium may in every case be defined to be "a property in virtue of which the medium transfers a small portion of motive energy from one part of the spectrum to another."

The media hitherto examined may be distinguished into two great classes—*Regular* and *Peculiar,* the former being considerably the larger of the two. As all media, however, are greatly affected by temperature, it may sometimes happen that a medium may be *regular* at one temperature and *peculiar* at another. These two classes shall be separately considered and described.

The following is a list of all the *regular* media hitherto examined :—

Solution of Nitrate of bismuth.	Calcareous spar, both rays.	Oil of anise, temp. 20° and 13°.
Water.	Oil of sassafras.	Creosote.
Sol. Subacetate of lead.	Sol. Nitrate of lead.	Crown-glass.
Sol. Nitrate of mercury.	Sol. Muriate of ammonia.	Rock-salt.
Sol. Sulphate of soda.		Arragonite, three axes.
Sol. Muriate of baryta.	Nitric acid.	Quartz, both rays.
Sol. Superacetate of lead.	Sol. Muriate of lime.	Sulphuret of carbon.
Sol. Nitrate of potash.	Sol. Potash.	Flint-glass.
Sol. Sulphate of magnesia.	Oil of turpentine.	Topaz, three axes.

In all *regular* media the transfer of motive energy takes place from the terminal to the central parts of the spectrum; and they are therefore *medio-positive.* The undulations corresponding to the lines D, E, and F are always *quickened* by the extrusive force; consequently their wave-lengths within the medium are *lengthened.* Hence for these three the formula is always $\dfrac{U}{\epsilon} - a + x = u.$ On the other hand, the undulations corresponding to the fixed lines B, C, G, and H are always *retarded,* or their wave-lengths within the medium are *shortened.* Hence for these four the formula is $\dfrac{U}{\epsilon} - a - x = u.$ In every case the motive energy gained by the one set of waves is exactly balanced by the loss sustained by the others; so that the sums of the positive and negative ex-

trusions being each denoted by X, these two quantities are always equal, and 2X may be regarded as the measure of the extrusive power of the medium.   This, which may be called "the law of equal transference," is the first to be recognized; and it is found to prevail in all media whatever, whether regular or peculiar.

From the above it follows that every *regular* medium presents two nodal points, at which the extrusion passes from positive to negative, or *vice versâ*—the upper node lying between C and D, the lower between F and G.   At these nodes the extrusion is *nil*.

The second law may in all *regular* media be expressed as follows: $3b_x + 2c_x - d_x = 3h_x + 2g_x - f_x$.   It is proposed to call this the "semel-bis-ter law."

From these two laws may be deduced the following general formula for expressing the extrusive power of a medium:

$$\frac{\mathrm{K}}{\epsilon} - (\mathrm{Q} + a \pm 2\mathrm{X}) = 0.$$

The two quantities K and Q have different values according as the medium is regular or peculiar.   In the former,

$$\mathrm{K} = (\mathrm{B} + \mathrm{C} + \mathrm{G} + \mathrm{H}) - (\mathrm{D} + \mathrm{E} + \mathrm{F}),$$

and is constant for all regular media.   In these, also,

$$\mathrm{Q} = (b + c + g + h) - (d + e + f) ;$$

so that the value of Q varies with the medium and temperature. The alterations in the constitution of these two quantities presented by peculiar media will be afterwards explained.

The third law governing the extrusions is as follows:—If these be taken in pairs equidistant from the centre e_x, and if the difference between b_x and h_x be denoted by δ_1, that between c_x and g_x by δ_2, and that between d_x and f_x by δ_3, then the differences between each pair of these three quantities δ_1, δ_2, and δ_3 will constitute a progression of the form ζ, 2ζ, 3ζ, the quantity ζ varying with the medium and temperature.   This it is proposed to call "the law of the equicentral common difference."   The varieties which this law presents are interesting; and it is important, as furnishing the means of detecting and correcting slight errors of observation.

There is yet a fourth law, which may be termed "the law of coincident nodes;" but for the understanding of its nature, a little explanation is required.

The series of internal wave-lengths b_2, c_2, d_2, based on the supposition of the medium being destitute of extrusive power, having been calculated from the formula

$$\frac{\mathrm{B}}{\epsilon} - a = b_2, \quad \frac{\mathrm{C}}{\epsilon} - a = c_2, \ \&\mathrm{c.,}$$

the series of refractive indices corresponding to it may be found from the formula

$$\frac{B}{b_2} = {}^{\mu_2}B, \quad \frac{C}{c_2} = {}^{\mu_2}C,$$

and so on. This series of indices, ${}^{\mu_2}B$, ${}^{\mu_2}C$, &c., is that which the medium would present had it only a refractive and dispersive, but no extrusive power. The differences between the observed indices and this second series show what portion of the former is due to the extrusive power of the medium. The positive and negative quantities do not here balance each other, as in the case of the wave-lengths; nor do they, like the extrusions, exhibit any symmetrical arrangement; but they present nodes corresponding to those of the extrusions. It might *à priori* be expected that this correspondence would be quite *exact*. It will accordingly be found to be *very nearly* so in every case, taking the indices as given by observation, or after having been subjected to the corrections required by the third law; and where the coincidence is not perfect, the difference is generally traceable to the defects of decimal calculation. There can therefore be no doubt of its being a general law that these two sets of nodes ought exactly to correspond; that is, the refractive indices of the nodes, deduced from the extrusions, should precisely tally with the nodes of the two sets of refractive indices.

To exhibit this fourth law, it is needful to determine the nodes of the wave-lengths and the nodes of the refractive indices. To find the nodes of the wave-lengths in a regular medium: Let c_x be the extrusion of c, and d_x the extrusion of d, then

$$c_x + d_x : c_x :: c - d : c - n_1,$$

whence n_1 is easily found. So from

$$f_x + g_x : f_x :: f - g : f - n_2,$$

n_2 may also be easily found. Then

$$c_x + d_x : c_x :: C - D : C - N_1$$

gives N_1, and

$$f_x + g_x : f_x :: F - G : F - N_2$$

gives N_2; whence should arise $\dfrac{N_1}{\epsilon} - a = n_1$ and $\dfrac{N_2}{\epsilon} - a = n_2$; also $\dfrac{N_1 - N_2}{\epsilon} = n_1 - n_2$. The nodes having been calculated and checked by these formulæ, the refractive indices of the nodes result from $\dfrac{N_1}{n_1} = {}^{\mu}N_1$ and $\dfrac{N_2}{n_2} = {}^{\mu}N_2$.

The nodes of the refractive indices ν_1, ν_2 will be found by a similar process. Here ${}^{\mu}C - {}^{\mu_2}C$ corresponds to c_x, and ${}^{\mu_2}D - {}^{\mu}D$

corresponds to d_x; whence we have

$$(^{\mu}C - {}^{\mu_2}C) + (^{\mu_2}D - {}^{\mu}D) : (^{\mu}C - {}^{\mu_2}C) :: {}^{\mu}D - {}^{\mu}C : {}^{\mu}C + \nu_1,$$

whence ν_1 may be found; so also

$$(^{\mu_2}F - {}^{\mu}F) + (^{\mu}G - {}^{\mu_2}G) : {}^{\mu_2}F - {}^{\mu}F :: {}^{\mu}G - {}^{\mu}F : {}^{\mu}F + \nu_2,$$

whence ν_2 may be found. Thence should arise respectively $^{\mu}N_1 = \nu_1$ and $^{\mu}N_2 = \nu_2$; that is, the refraction of the node should be the same as the corresponding node of the refractions.

When the corrections on the refractive indices, and the corresponding extrusions required by these laws, shall have been made where necessary, there will be obtained a series of values of these two sets of quantities perfectly self-harmonious, all the errors arising from inaccuracy of observation having been made to neutralize each other. The resulting figures will be found to agree so nearly with those obtained from observation, as to leave not a doubt of the real existence of the several laws which have been thus ascertained, and of the accuracy of the method of investigation pursued.

So much for the *regular* media, which all present the foregoing characteristics; and now for the *peculiar*. The only media yet ascertained to fall under this class are the following eight :—

Alcohol.	Pyroligneous acid.
Oil of cassia.	Muriatic acid.
Oil of anise, T. 15°.	Solution of soda.
Muriate of zinc.	Sulphuric acid.

The peculiarities presented by these media, as respects their extrusive property, are of three kinds :—1st, an alteration in the position of the nodes; 2nd, an alteration in the character of the transfer of motive energy; and 3rd, an alteration in the number of the nodes, involving, as a consequence, both the other two peculiarities.

Alcohol has its upper node between B and C, its lower between F and G; it may therefore be termed *high nodal*. Oil of cassia has its upper node in the usual place between C and D, but its lower between G and H; it may therefore be termed *low nodal*. Oil of anise, at temp. 15°, is also *low nodal*; but at the other two temperatures examined it is *regular*. The muriate of zinc presents two peculiarities. It is, like alcohol, *high nodal*; but it is also *medio-negative*; that is, the transference of motive energy is from the central to the terminal parts of the spectrum, being the reverse of what is observed in regular media. Its nodes are between B and C and E and F, near F.

The remaining four present more than two nodes, thus involving an oscillation of the transfer of motive energy. Pyroligneous acid has three nodes—between B and C, C and D, G and

H near G, so that the first is the abnormal node. The transfer of energy takes place from C and H to B, D, E, F, and G.

Muriatic acid has four nodes—between B and C, C and D, D and E, F and G, the transfer of energy being from B, D, G, H to C, E, F.

Solution of soda presents also four nodes—between B and C, C and D, E and F, G and H. The transfer of energy is from B D E H to C F G.

Sulphuric acid has no less than five nodes—between B and C, D and E, E and F, F and G, G and H. The transfer of energy is from B E G to C D F H. This medium has the smallest amount of extrusive power of any yet examined.

It would be well to have the observations on these eight media carefully repeated, in order to ascertain how far these peculiarities, or any of them, may be due to serious errors of observation,—a supposition which will in the sequel be shown to be very highly probable. At the same time it would be difficult to assign any reason why such peculiarities should not exist, or why all media whatever should conform to one uniform type.

Meanwhile, assuming the observations to be nearly correct, it is needful to point out the changes which these several peculiarities introduce into the laws before indicated.

1st. The law of equal transference subsists unaltered.

2nd. The semel-bis-ter law undergoes the following modifications. The regular type being

$$3b_x + 2c_x - d_x = 3h_x + 2g_x - f_x,$$

it becomes in high nodal media,

$$3b_x^{\cdot} - 2c_x^{\cdot} - d_x = 3h_x + 2g_x - f_x;$$

in low nodal media,

$$3b_x + 2c_x - d_x = 3h_x - 2g_x - f_x;$$

in pyroligneous acid,

$$-3b_x^{\cdot} + 2c_x - d_x = 3h_x - 2g_x - f_x;$$

in muriatic acid,

$$3b_x - 2c_x + d_x = 3h_x + 2g_x - f_x;$$

in solution of soda,

$$3b_x - 2c_x + d_x = 3h_x - 2g_x - f_x;$$

in sulphuric acid,

$$3b_x - 2c_x - d_x = -3h_x + 2g_x - f_x.$$

Hence the general expression for this law must be

$$\pm 3b_x \pm 2c_x \pm d_x = \pm 3h_x \pm 2g_x \pm f_x.$$

3rd. As respects the law of the equicentral common difference, one general rule will suffice. Where the pairs b_x and h_x, c_x and

g_x, d_x and f_x have *like* signs, as in regular media, their *differences* are to be taken to form the three quantities δ_1, δ_2, δ_3, whose three differences form the required progression. But if the members of any of the above pairs have *unlike* signs, then their *sum* is to be taken instead of their difference. Thus if b_x and h_x stand on opposite sides of the account, we must take the *sum* instead of the difference of this pair to constitute the quantity δ_1; and so with the others.

4th. The law of coincident nodes is not affected by these peculiarities,—the indices of the nodes, and the nodes of the indices always coinciding very nearly, whatever may be the position or number of the nodes.

With respect to the general formula for expressing the extrusive power of a medium, namely $\dfrac{K}{\epsilon} - a + Q \pm 2X = 0$, the constitution of the quantities K and Q is materially modified by these peculiarities. As a general rule, all the fixed lines which undergo *positive* extrusion are to be added together in one sum, and all those *negatively* extruded into another, and the difference between those two sums will be K. Thus in *high* nodal media we have

$$(C + D + E + F) - (B + G + H) = K_2;$$

and in *low* nodal,

$$(D + E + F + G) - (B + C + H) = K_3;$$

and so with the others.

In like manner, those of the quantities *b, c, d,* &c., which are *positively* extruded, are to be collected into one sum, and those *negatively* extruded into another, and the difference between those two sums will be Q.

Thus in *high* nodal media we shall have

$$(c + d + e + f) - (b + g + h) = Q_2;$$

in low nodal,

$$(d + e + f + g) - (b + c + h) = Q_3.$$

The alterations introduced by an increase in the number of the nodes will be easily understood from the above.

The calculations from which the foregoing laws have been deduced have been based on the *relative* normal wave-lengths for the fixed lines, assuming that of B as unity, according to the values given in the separate paper on that subject*. These values, with their logarithms, stand as under:—

B.	C.	D.	E.	F.	G.	H.
1·000000,	0·953893,	0·856059,	0·764567,	0·704210,	0·623398,	0·570655,
0·0000000,	$\bar{1}$·9794999,	$\bar{1}$·9325036,	$\bar{1}$·8834154,	$\bar{1}$·8477024,	$\bar{1}$·7947653,	$\bar{1}$·7563732.

* This paper will be given in a subsequent Number.—Ed.

From a considerable number of trials made with various sets of normals, it appears that any alteration on the above numbers, within the probable limits of error, would not affect the general character of the laws above indicated.

From the foregoing investigation it follows that the refractive index deduced from observation for any of the fixed lines, is a somewhat complex quantity. In the first place, each index involves a certain fixed amount ϵ, which is constant for waves of every length. It is the common divisor by which the differences between the normal wave-lengths would have to be divided, in order to produce within the medium a set of wave-lengths which should present no extrusion of the fixed lines, but in which each line should occupy the same position in relation to the others as the normal lines occupy in the spectrum produced by transmitting a divergent beam through a system of equidistant fine lines. This quantity ϵ is always less than the observed refractive index; so that it forms only a portion, yet by much the greater proportion of its amount. Had all the fixed lines this constant ϵ as their common refractive index, the medium would then have refractive power without either dispersive or extrusive power. Although no single medium exhibits this peculiarity, it is possible by a combination of two or more substances to obtain a compound medium that shall present this condition, which is that of achromatic refraction. The preceding investigation, if followed out, may tend to facilitate the effecting of such combinations.

As the purely refractive power of a medium is due simply to the state of compression of the æther within its pores, the constant ϵ may be viewed as the measure of that compression, and may accordingly be termed the compressive index of the medium, as distinct from the refractive index, which is a complex quantity; or it might be termed the optical elasticity of the medium, for it is at least a measure of that elasticity. In doubly-refracting media the value of ϵ differs considerably in the direction of the different optic axes of the same medium, thus showing the compressive power of the constituent molecules to be specific and polarized.

The variety in the refractive indices of the fixed lines in any medium is due primarily to its dispersive power. The effects of this property, viewed apart from the extrusive power, are exhibited by the series of indices $\mu_2 B$, $\mu_2 C$, &c. obtained from the formulæ $\dfrac{U}{\epsilon} - a = u_2$ and $\dfrac{U}{u_2} = \mu_2$, in which only the normals and the two constants ϵ and a are involved. The differences between this series of indices and the constant ϵ may accordingly be viewed as the respective *dispersive* indices of the fixed lines in

the particular medium. The constant a may be termed "the optical abstract," because it must be taken from the normal wave-length corresponding to each of the fixed lines after it has been divided by the constant ϵ, in order to obtain the internal wave-lengths b_2, c_2, &c.

The quantity a is thus indicated as being a portion of the refracted wave-lengths, distinguishable from the main body, and of the same magnitude for all waves. But while it is thus constant for the same medium and temperature, yet in comparing one medium with another, the value of a will depend on the constant ϵ, and on s the sum of the internal wave-lengths jointly; for S being the sum of the normal wave-lengths, the value of a is $= \dfrac{\dfrac{S}{\epsilon} - s}{7}$. The constants ϵ and a are thus mutually independent, inasmuch as a may be indefinitely altered without affecting ϵ, and *vice versâ*.

The product of these two constants, or ϵa, deducted from each of the normal wave-lengths, will show the extent to which each normal is shortened during its passage through the medium, in virtue of the dispersive power alone. The *actual* loss of length being represented by $a\epsilon$, is of the same magnitude for all waves; but it of course tells more on those waves which are primarily shorter. Hence the numbers representing the loss of length sustained by each wave in proportion to its primary length, from the operation of the dispersive power of the medium alone, irrespective of either its refractive or extrusive powers, are in exact inverse proportion to the primary wave-lengths.

Thus, taking as an example the bisulphuret of carbon (a medium of high dispersive power), its constant a is $0{\cdot}038772$, and $\epsilon a = 0{\cdot}058953$, which, being deducted from each normal wave-length, gives as under:

B 1·000000	C 0·953893	D 0·856059	E 0·764567	F 0·704210	G 0·623398	H 0·570655
0·058953	0·058953	0·058953	0·058953	0·058953	0·058953	0·058953
0·941047	0·894940	0·797106	0·705614	0·645257	0·564445	0·511702

Dividing these remainders by the normal wave-lengths, we obtain

B 0·941047, C 0·938197, D 0·931135, E 0·922894, F 0·916285, G 0·905433, H 0·896693,

which numbers represent the reduced wave-lengths, reckoning each wave as unity; consequently the complements of these numbers, being

B 0·058953, C 0·061803, D 0·068865, E 0·077106, F 0·083715, G 0·094567, H 0·103307,

represent the loss of length sustained by each wave, in propor-

tion to its primary length, from the operation of the *dispersive* power of the medium *alone*; and these are in *inverse* proportion to the primary wave-lengths. Thus also the proportion of the refractive indices corresponding to this temporary loss of wave-length, must also have a certain dependence on the initial force which generated the particular wave to which the index belongs, and may be found by multiplying the indices $^\mu B_2$, $^\mu C_2$, &c. (being the observed indices freed from the portion due to the extrusion) by the above complementary numbers, representing the loss of length sustained by the waves from the operation of the disper-sive power. Thus, in the case of the bisulphuret of carbon, the indices $^\mu B_2$, $^\mu C_2$, &c. are

$$^\mu B_2 \ 1{\cdot}615760, \quad ^\mu C_2 \ 1{\cdot}620667, \quad ^\mu D_2 \ 1{\cdot}632958, \quad ^\mu E_2 \ 1{\cdot}647542, \quad ^\mu F_2 \ 1{\cdot}659425,$$
$$^\mu G_2 \ 1{\cdot}679311, \quad ^\mu H_2 \ 1{\cdot}695681.$$

These, multiplied by the above series of complementary numbers, give for the proportion of the indices due to the dispersive power,

$$B \ 0{\cdot}095255, \quad C \ 0{\cdot}100162, \quad D \ 0{\cdot}112453, \quad E \ 0{\cdot}127037, \quad F \ 0{\cdot}138920, \quad G \ 0{\cdot}158806,$$
$$H \ 0{\cdot}175176,$$

which numbers are identical with the differences between the above indices and the constant $\epsilon = 1{\cdot}520505$.

In *different* media, the loss of length sustained by any one wave through the action of the dispersive power is always pro-portional to the constant a, which may be accordingly regarded as a measure of that loss.

To generalization beyond this point, the *uncompensated* errors of observation and the yet unascertained effects of change of tem-perature are a serious obstacle. But the data already obtained may be found useful in detecting some of those errors and effects, and in determining their probable limits.

As regards the effects of temperature, the most instructive cases are those of the oils of cassia and anise; for of these we have observations at three different temperatures, though unfor-tunately these do not coincide in the two media. Comparing the values of ϵ in these two fluids for the three sets of observa-tions, they will be found as under :—

	Oil of Cassia.		Diff.		Oil of Anise.		Diff.
No. 1. Temp.	10°	$\epsilon = 1{\cdot}477740$	. 2478	No. 1. Temp.	13°·2	$\epsilon = 1{\cdot}478492$	. 989
2. „	14°	1·475262	. 5726	2. „	15°·1	1·477503	. 4044
3. „	22°·5	1·469536	. 8204	3. „	20°·9	1·473459	. 5033

It will be perceived that not only are these values of ϵ in the *inverse* order of the temperatures, but that their differences are not far from being proportional to the differences of temperature. To make them exactly so, they would have to be altered thus :—

	Oil of Cassia.		Diff.		Oil of Anise.		Diff.
No. 1.	$\epsilon = 1\cdot477811$	. .	2621	No. 1.	$\epsilon = 1\cdot478606$	. .	1217
2.	$1\cdot475190$	. .	5569	2.	$1\cdot477389$	. .	3813
3.	$1\cdot469621$	. .	8190	3.	$1\cdot473576$	. .	5030

The above correction may be effected by an alteration of the indices so small as to be of no account.

This law, then, that the indices of elasticity of the æther in the pores of any medium are in the inverse order of the temperatures, and the differences of the indices are proportional to the differences of temperature, may be regarded as rendered highly probable by these two cases, being the only media on which we have observations at more than two temperatures, so as to illustrate this point.

The subsistence of this law in the case of oil of anise is all the more remarkable, because the indices of *all* the fixed lines are there greater in No. 2 (the intermediate temperature) than in either No. 1 or No. 3, thus showing the *absolute* magnitudes of these indices alone to be an imperfect criterion by which to judge of the condition of the æther within the pores of the medium. This law thus removes an anomaly which would otherwise be presented by the oil of anise, in which, were we to judge by the indices of the fixed lines alone, we should be led to infer that, in passing from temperature $13^\circ\cdot25$ to temp. $15^\circ\cdot1$, the enlargement of the pores is attended by an increase in the tension of the æther,—a result in the highest degree improbable. The foregoing investigation shows that this is not the case, but that the tension of the æther, as determined by the value of ϵ, does actually diminish with the enlargement of the pores, consequent on the rise of temperature.

This law is important in reference to the undulatory theory, being exactly what it would lead us to expect; and it confirms the conclusion that the quantity ϵ is the true index of the elasticity of the æther within the pores of the medium.

On comparing together the two media—the oils of cassia and anise—it will be perceived that in the former the rise of $12^\circ\cdot5$ of temperature, from temp. 10° to temp. $22^\circ\cdot5$, gives on the value of ϵ a decrease of $0\cdot008190$; and in oil of anise, the rise of $7^\circ\cdot65$ from temp. $13^\circ\cdot25$ to $20^\circ\cdot9$ gives on ϵ a decrease of $0\cdot005030$. These two are so nearly proportional to each other, as to lead to the inference that in two different media, in which the elasticity of the æther is nearly the same, the effects produced by a given change of temperature are also nearly the same.

From the foregoing, it appears that the effects of temperature in altering the action of any medium on the light passing through it are so considerable, as to render it highly desirable that observations should be made on each medium at six or seven different

temperatures, in order that these might operate as a check on each other.

It will be particularly noted that in each medium the constants ϵ and a are independent of the *absolute* magnitudes of the extrusions, and are affected only by the *relations* which these individually bear to each other. Hence, provided those relations be preserved, the constants ϵ and a will remain unaffected by any alteration in the *absolute* magnitudes of the extrusions, which may accordingly be multiplied by any multiple m, integral or fractional, without altering ϵ or a. These two quantities are thus consistent with an indefinite number of sets of indices of refraction, so that these last may always be altered *in a certain manner* without affecting those constants.

This point being kept in view, the following general formula will be found applicable to all media whatever, namely,

$$\epsilon a \left\{ \frac{B}{(B-\epsilon b)\pm\eta} + \frac{C}{(C-\epsilon c)\pm\eta} + \frac{D}{(D-\epsilon d)\pm\eta} + \frac{E}{(E-\epsilon e)\pm\eta} \right.$$
$$\left. + \frac{F}{(F-\epsilon f)\pm\eta} + \frac{G}{(G-\epsilon g)\pm\eta} + \frac{H}{(H-\epsilon h)\pm\eta} \right\} = S,$$

the quantities ϵa and η being each constant for the same medium and temperature, and S being the sum of the normal wavelengths, or the total amount of *vis viva* involved, the conservation of which thus depends on these three constants. To find the constant η, if we call the sum of the series $\dfrac{B}{B-\epsilon b} + \dfrac{C}{C-\epsilon c}$ + &c. $= \Sigma$, and call $\dfrac{S}{\Sigma} = \epsilon a'$, then η is the difference between ϵa and $\epsilon a'$. If $a > a'$, then the sign of η is $+$; if $a' > a$, the sign of η is $-$, and in either case is constant for the medium and temperature.

Now the value of η depends on the relation of X (the sum of the positive or negative extrusions) to a; and there may always be found for each medium and temperature such a positive value of X as shall make $\eta = 0$. This it is proposed to call the *limiting* value of X, and to denote it by X'. In some media this limiting value nearly coincides with the actual value of X, as given by observation; in others the actual value is several times greater than the limit; while in a few it falls somewhat below it. Calling $\dfrac{X'}{a} = \omega$, it will be found that, making a small allowance for the effects of errors of observation, this quantity ω is constant for all media whatever; so that in every instance we have $a\omega = X'$, the limiting value of the extrusions. This constant ω may be

found from the following formula,

$$\frac{4(B+C+G+H)-3(D+E+F)}{S}=\omega,$$

and its logarithm is $\bar{2}\cdot4216417$.

With a view to a further generalization, it is needful to examine the effects produced on the extrusions by raising the normal wave-lengths of the fixed lines to different powers, and dividing these by the indices of refraction. Selecting for this purpose the medium flint-glass No. 30 of Fraunhofer, the observed indices of which are pretty nearly accurate, it will be found that, while with the first powers of the normals the extrusions are

B −0·000419, C −0·000159, D +0·000277, E +0·000468, F +0·000422,
G −0·000047, H −0·000542, S +0·001167,

with the squares of the normals they are

B −0·000049, C −0·000025, D +0·000031, E +0·000063, F +0·000068,
G −0·000026, H −0·000062, S +0·000162,

and with the cubes they become

B +0·000184, C +0·000027, D −0·000148, E −0·000159, F −0·000096,
G +0·000020, H +0·000172, S +0·000403.

It will be observed that in this last series the extrusions have changed their signs, and are greater in amount than with the squares. There must accordingly be an intermediate exponent of the normals between 2 and 3, at which the extrusions will be reduced to their lowest amount. This exponent of least extrusion will be found to be, for flint-glass No. 30, as nearly as possible 2·2, with which the extrusions become

B +0·000007, C −0·000009, D −0·000010, E +0·000004, F +0·000024,
G −0·000017, H +0·000001, S +0·000036.

These values are so insignificant that they may be regarded as arising from small errors of observation, and they may be entirely thrown out of view in the calculation of the indices. The following are the differences between the indices thus calculated and those given by observation:—

B −0·000020, C −0·000026, D −0·000038, E +0·000035, F +0·000138,
G −0·000130, H +0·000007.

These differences are so small as to lie quite within the limits of probable error in the observed indices.

Now what is thus true of flint-glass No. 30, will be found to hold good with respect to all other media. Each has a specific exponent of least extrusion, which is constant for the medium and temperature. The question thus arises, How is this expo-

nent of least extrusion to be determined? On a careful analysis of all the media, it will be discovered that the value of this exponent depends entirely on the proportion which the extrusive property of the medium bears to its dispersive power at a given temperature; in other words, it depends on the proportion which the *irrationality* bears to the length of the spectrum, with a given prism and at a given distance from the prism. Representing the dispersive power by the optical abstract a, and the irrationality by the amount of the positive and negative extrusions $2X$, and calling $\frac{2X}{a} = \rho$ the ratio which the extrusion bears to the dispersion—representing also the exponent of least extrusion by n, we have the following equation universally applicable,

$$\frac{\rho}{n-1} = \text{constant.}$$

The value of this constant, as determined from the best of the observations, appears to be as nearly as possible $0 \cdot 0092593$*; at least this value is sufficiently near the truth for all practical purposes. The reciprocal of this number is $10 \cdot 8$, which, added to unity, gives $11 \cdot 8$ as the highest limit of these exponents, or that which the medium would have if ρ were $= 1$, or $2X = a$. The lower limit of these exponents, being 1, obtains when a is equal to the above constant, or $a = 0 \cdot 009259$ and $2X = 0$.

As ρ is obtainable with tolerable correctness from any set of observations *which are approximately accurate*, the exponent of least extrusion may always be found from the equation $10 \cdot 8\rho + 1 = n$, for any medium and temperature. It is unnecessary, in estimating these exponents, to go beyond the first place of decimals, which gives their value sufficiently near for the purposes of calculation.

The exponents calculated from this equation for the various media will be found specified in Table I. From this specification the muriate of zinc is excluded, because it forms an exception. This circumstance, however, need not lessen confidence in the correctness of the law; for it only tends to confirm the opinion expressed by the observer himself, that the indices which he has given for this medium are so inaccurate, that no conclusion can be formed with respect to it till further observations be made.

The exponents of least extrusion having been thus ascertained from the observed indices of refraction, the next step is, by means of the exponent, so to correct the indices as to reduce the extrusions to zero—a matter of easy accomplishment; for the extrusions being thus eliminated from the calculation, the formula

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* This value is of course open to future correction.

for expressing the relation of the primary wave-length of any of the fixed lines to its index of refraction, becomes universal and quite simple. Denoting the wave-length by λ, and the corresponding index of refraction by μ, we have in every case

$$\mu = \frac{\lambda^n}{\dfrac{\lambda^n}{\epsilon_n} - a_n} \; ;$$

where the exponent n is constant for the medium and temperature, as are also the quantities ϵ_n and a_n, being the index of elasticity and the optical abstract corresponding to that exponent. These two are ascertainable from the observed indices, in the same manner as are ϵ and a for the first power of the normals, and they will be found specified for each medium in Table I.

The indices of refraction for the various media, as calculated from this general formula, are given in Table VI., while the observed indices are specified in Table VII. The differences between the calculated and observed indices are exhibited in Table VIII.

[To be continued.]

XXIII. *On a Simple and Expeditious Method of estimating Phosphoric Acid and its Compounds, which is particularly applicable to the Analysis of Phosphatic Manures and the Ashes of Plants. By* Edmund W. Davy, *M.B., M.R.I.A. &c., Professor of Agriculture and Agricultural Chemistry to the Royal Dublin Society*[*].

THE want of some simple and expeditious method of estimating phosphoric acid and its compounds has long been felt; for though several means have been devised for the determination of this acid under different circumstances, they are so complicated and require so much time in their performance, that they are quite unsuited for many cases where expedition is particularly required.

After much investigation I have succeeded in devising a method which is very quickly performed, easy of execution, and capable of affording extremely accurate results. It is founded on the fact that phosphoric acid possesses a great attraction for the peroxide of iron, so that when a persalt of that metal is added to a solution containing phosphoric acid, an insoluble combination of the peroxide of iron with that acid is produced, which under particular circumstances has the following com-

position, viz. ($Fe^2 O^3, PO^5$), in which 80 parts of the peroxide containing 56 of metallic iron, are combined with 72 parts of phosphoric acid.

The fact of the peroxide of iron forming an insoluble compound with phosphoric acid has long been known; and different methods founded on it have been proposed and adopted for the estimation of phosphoric acid and its compounds,—as, for example, those of Berthier, Kobell, and Raewsky, which are described in different works on analytical chemistry.

Each of those methods, however, requires a considerable devotion of time, from the collecting, washing, drying, igniting, weighing, and other operations through which the precipitated phosphate of iron has to pass, which not only consume much time, but, unless they are very carefully performed, lead to great inaccuracies in the results obtained.

In the modification I propose, I dispense altogether with those tedious and troublesome operations, by simply adding a graduated solution of iron of known strength to the phosphate, and ascertaining the point when sufficient iron has been added to combine with all the phosphoric acid present; and from the quantity of iron employed, I calculate the amount of that acid; every 56 parts of iron being equivalent to 72 of phosphoric acid.

The iron solution which I use for this purpose is somewhat different from that hitherto employed. I make it in the following manner: a certain quantity of the finest pianoforte iron wire, perfectly clean and free from rust, is dissolved in pure hydrochloric acid, and sufficient nitric acid is afterwards added to convert the so-formed protochloride into the perchloride of iron; and as any excess of hydrochloric acid would be injurious to the process, and as it cannot be removed by heating the mixture and evaporating it to dryness, which would decompose a portion of the perchloride, giving rise to peroxide of iron and hydrochloric acid, I add caustic ammonia till all the free acid has combined with that substance, and a small quantity of the peroxide of iron precipitated by the alkali remains undissolved after agitating the mixture and allowing it to stand for a few minutes. Acetic acid is then added to dissolve the oxide, and when it has effected its complete solution (which it will do by leaving the acid to act on the oxide at the ordinary temperature for a short time), the mixture is largely diluted with distilled water and graduated in the usual way, so that the amount of iron may be known which is contained in a given quantity of the liquid. The proportions which I have used are 100 grains of iron in 1000 cubic centimetres of the liquid; and this quantity of standard solution will suffice for a great number of determinations.

This liquid, which contains the perchloride and peracetate of iron, together with the chloride of ammonium, the acetate of ammonia, and a little free acetic acid, I find to be well adapted for the estimation of phosphoric acid ; and as it appears from my experiments that it may be kept for a considerable time without undergoing any change, it is therefore preferable to the acetate and other salts of iron hitherto employed, which, from their proneness to decompose, require to be freshly prepared before using them. Having made the standard solution of iron, the next step is to prepare the phosphate, and if it is (as is generally the case) an insoluble one dissolved by an acid, ammonia is first added to the solution till it is distinctly alkaline to turmeric paper*, then acetic acid to redissolve completely the phosphate precipitated by the ammonia, and leave that acid in slight excess ; and finally the standard solution of iron is carefully added from a Mohr's alkalimeter, or any other convenient form of volumetric apparatus†, till the iron begins to be in slight excess.

I ascertain this point by taking out of the mixture (after it has remained a few minutes with occasional stirring to effect the complete combination of the oxide of iron with the phosphoric acid) a drop of the solution on the end of a glass rod, and touching with it a piece of thick and close-textured filtering paper, under which is placed some ordinary filtering paper, which has been previously saturated with a strong solution of gallic acid, and then dried.

By this arrangement we avoid filtering : the insoluble phosphate of iron formed in the process being retained by the upper paper, and the solution passing down to the lower, at once shows, by the light-purplish stain produced, the point when sufficient iron has been added to combine with all the phosphoric acid present, and a very minute excess exists in the mixture‡.

This experiment being repeated a second or third time, by having the phosphate under examination dissolved in a given quantity of solution and taking a certain amount of it for each

* A large excess of ammonia should be avoided, for by its afterwards combining with the acetic acid to form the acetate of ammonia, which dissolves to a slight extent the phosphate of iron, the amount of phosphoric acid estimated in that case by this process is somewhat diminished. A large excess of acetic acid, however, appears to affect but very slightly the results obtained.

† The form which I have used and found most convenient is Mohr's alkalimeter with the addition of Professor Erdmann's float, which affords great facilities for the accurate reading of the volume of liquid employed.

‡ When the excess of iron added is very minute, I have found that by gently drying the wet spot on the gallic acid paper, the stain becomes more visible.

determination, we ascertain the exact quantity of iron solution, and therefore of iron, necessary to produce this effect; and from this we easily calculate the amount of phosphoric acid present in the manner before explained.

Though the ferrocyanide and the sulphocyanide of potassium may be substituted for the gallic acid, using them in the way I have described for that acid, still I have found gallic acid to give more satisfactory results than either of them, and to be an exceedingly sensitive test to the presence of iron in solution. For I found, by direct experiment with 2 cubic centimetres of the iron solution containing 0·2 parts of a grain of iron, diluted with distilled water so as to make up 300 cubic centimetres, that one drop of this mixture let fall on the gallic acid paper, produced a faint purplish stain, and the smallest drop which could be taken up on the top of a glass rod gave with a solution of gallic acid a very decided effect; so that this test is one of great delicacy in ascertaining the point when the slightest excess of iron has been added to the mixture in this method for the estimation of phosphoric acid and the phosphates. I may observe that when the phosphate under examination is very largely diluted with water, it will lead to more accurate results to reduce by evaporation the bulk of the liquid (having previously added a little hydrochloric acid in those cases where evaporation would cause the precipitation of any insoluble phosphates),—the effect of large dilution, by its rendering the gallic acid less sensitive to the point when iron is in excess, being to indicate a somewhat greater amount of phosphoric acid than is present.

This source of inaccuracy might also in a great measure be obviated by seeing how much of the iron solution was necessary to give an indication of that metal when it was diluted with a bulk of fluid equal to that employed in the experiment; and this being deducted from the quantity of iron necessary in the determination, would give a very close approximation to the real amount required to combine with the phosphoric acid, where it might be inconvenient and occupy too much time to evaporate the liquid.

Hitherto the estimation of phosphoric acid volumetrically by the use of a standard solution of iron has been thought by many to be impracticable, as the analyses of different chemists show that the composition of phosphate of iron is subject to great variation.

This, however, may be accounted for by the different circumstances under which it has been formed by those experimenters, which give rise to phosphates of different constitution. But I entertain no doubt that, by always placing the oxide of iron and the phosphoric acid under the same conditions, compounds of the same constitution would in every case be formed.

Be this as it may, my experiments have clearly shown me that under the conditions in which I place those substances, in the method recommended for the estimation of phosphoric acid, a compound having the composition $(Fe^2 O^3, PO^5)$ is uniformly produced. I have proved this by taking certain quantities of different phosphates and treating them in the manner described, I ascertained how much iron was necessary to combine with the phosphoric acid present; and in every case I have obtained results which agree almost exactly with those I should have got, calculating according to that formula, which would not have been the case had the composition of the phosphate of iron been different.

The following are taken from among my experiments. Some pyrophosphate of magnesia and tribasic phosphate of lime being carefully prepared, a certain quantity of each was taken and dissolved by the aid of heat in a little hydrochloric acid; and the solutions being diluted with distilled water, they were very carefully graduated, so that 5 cubic centimetres should contain one grain of each of those compounds.

Common tribasic phosphate of soda was likewise taken, and after being exposed to a red heat for some time to convert it into the anhydrous pyrophosphate, a solution of it was also made, containing the same proportion of dry salt as in the former cases.

Five cubic centimetres of each solution (containing one grain of those compounds) were then taken and several estimations made, employing the same quantity of solution every time, when the results obtained were as follows :—

Amount of iron required to combine with the phosphoric acid contained in one grain of—

	By Calculation, parts of a grain.		By Experiment, parts of a grain.
Pyrophosphate of magnesia	0·5000	1st Experiment	0·5000
	,,	2nd ,, ·	0·5000
	,,	3rd ,,	0·5000
	,,	4th ,,	0·5000
Tribasic phosphate of lime	0·3589	1st ,,	0·3600
	,,	2nd ,,	0·3600
	,,	3rd ,,	0·3600
	,,	4th ,,	0·3600
Anhydrous pyrophosphate of soda*	0·4179	1st ,,	0·4200
	,,	2nd ,,	0·4200
	,,	3rd ,,	0·4200
	,,	4th ,,	0·4200

* In the case of the bibasic salts, it is necessary to convert them into the tribasic before the addition of the iron solution; this I have found (in the

These results, agreeing so closely with those obtained by calculation, prove that such a compound of the oxide of iron and phosphoric acid was produced, otherwise the calculated amount of iron would have been very different from that obtained by experiment.

They also show how very accurate this method is, and how constant are the results obtained by its adoption ; and the ease and expedition with which the estimation of phosphoric acid is effected, renders it a very useful means for the determination, not only of that acid itself, but likewise for that of many of its compounds, which can easily be calculated from the amount of phosphoric acid present.

I have already found that it is particularly useful in estimating the quantity of soluble and insoluble phosphates in superphosphate, a manure the analysis of which has hitherto been attended with considerable trouble. And from my experiments I have no doubt that it will be found to be extremely useful in the estimation of the phosphates in different manures, the ashes of plants, and many other cases of common occurrence where an expeditious determination of the quantity of those substances is required.

Laboratory of the Royal Dublin Society,
      February 3, 1860.

XXIV. *On the Composition of the Photographic Image.*
By John Spiller, *F.C.S., of the War Department*[*].

THE composition and chemical nature of the photographic image, as produced by the action of light upon the chloride of silver, is even at the present moment, notwithstanding the numerous experiments recorded on the subject, one upon which authorities are divided. While there is abundant evidence to show that the darkening consequent on exposure to the sun's rays is a process of reduction accompanied with the evolution of chlorine, there are yet two opinions entertained as to the *extent* to which this reducing action ordinarily proceeds. In accordance with one hypothesis, the white or protochloride of silver ($Ag Cl$) is assumed to suffer the full decomposition into its elements,

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two cases I have tried, viz. the magnesia and soda salts) to be easily effected by heating them for a few minutes with a little hydrochloric acid. But in these as in other cases, the solution of the phosphate must be suffered to cool to the ordinary temperature before the estimation of the phosphoric acid is attempted, as heat alters the conditions, and appears to give rise to a different compound of the oxide of iron and phosphoric acid.

  [*] Communicated by the Author.

becoming therefore reduced to the *state of metal* ; while according to a second view, the progress of this reducing action is limited to an intermediate stage, whereby a compound is produced containing less chlorine by one-half than the original substance, and to which the name and formula, subchloride of silver ($Ag^2 Cl$), have been applied. As a contribution towards a fuller explanation of the chemical changes involved, I beg to submit the following results of a series of experiments, made at intervals of leisure during the summers of 1857, 1858, and 1859, which would appear strongly to favour the first-mentioned hypothesis.

Preliminary experiments upon the freshly precipitated chloride of silver, as ordinarily prepared, having demonstrated the difficulty of effecting more than a mere superficial decomposition by exposure to sunlight, a process of preparation was adopted whereby an exceedingly finely divided condition of the substance was ensured, and its exposure conducted under circumstances favourable to its thorough decomposition. For this purpose highly dilute solutions were prepared, both of nitrate of silver and chloride of sodium, in proportions so adjusted that equal bulks represented amounts of chlorine and of silver in the ratio of their chemical equivalents.

(5·85 grains of pure rock-salt, on the one hand, and 17 grains of fused and neutral nitrate of silver, were dissolved each in two gallons of pure distilled water.)

When equal measures of these solutions were mixed in an obscurely illuminated apartment, the white chloride of silver was precipitated in a form so finely divided that an opalescence only, without visible particles, was at first apparent. By diffused daylight this became quickly darkened, and in the course of time subsided into a very small purple-grey deposit. But in order to ensure full decomposition, it was the general practice to employ the silver solution in excess and to add the salt water under the full action of sunshine, the liquid being contained in three, and sometimes four, pale glass flasks, each of nearly two gallons capacity, which were placed on the roof of one of the buildings in the Royal Arsenal, Woolwich, and in such a position that the solar rays had uninterrupted access to their contents, almost from sunrise to sunset. Under favourable circumstances it was then frequently impossible to observe the formation of the white chloride of silver on mixing the two solutions, so rapidly was it converted into the dark coloured product. At the expiration of the day's action the small precipitate had become completely darkened, and in the same time had subsided, so that on the following morning the supernatant liquid could be drawn off through a siphon, and a fresh charge introduced, the pro-

duct being usually collected from the flasks at intervals of two or three days.

In this manner, during the remarkably brilliant days in June and July 1857, no less than forty-six gallons of the standard solutions were submitted to treatment, and a comparatively considerable quantity of the darkened material procured for investigation.

The appearances presented during these trials were often such as to indicate a reduction more complete than that which would probably be required on the subchloride hypothesis :—first, a thin pellicle of high metallic lustre, white as silver, was usually seen floating on the surface of the liquid, and the internal walls of the flasks were frequently coated with a film much resembling the condition of silver reduced by any of the so-called " silvering" processes.   The product also, although containing admixed chloride, was susceptible of a high degree of lustre on being burnished in an agate mortar.

In colour the products of several experiments varied a little,— sometimes presenting a dark purple-grey appearance, at other times the grey was slightly tinged with green ; and to this depth of colour is probably attributable the circumstance that sometimes, during exposure to the burning rays of a midsummer sun, the contents of the flasks attained a degree of temperature which should be recorded as a condition of experiment, although such heat may not be supposed to have exercised any special influence in determining an abnormal decomposition.   On the 24th of June, 1857, a hot sunny day with cloudless sky, the temperature of the dark liquids in three of the flasks reached 110°, 115°, and 116° Fahr. respectively ; whilst at the same time, 3 p.m., a thermometer in the shade registered 83°, and only 91° with the sun shining freely on the mercury in the bulb ; such high degrees of temperature were, however, unusual and never again observed.

With a view to counteract the possibility of the material undergoing alteration by drying, it was constantly preserved under water, and in the moist condition submitted to numerous experiments for the purpose of ascertaining its constitution.

On determining by chemical analysis the composition of an average product, it was found to contain a larger proportion of silver than the original white chloride, as will appear in the following comparison :—

	Composition of white chloride.	Found in grey product.
Silver . . .	75·26	81·0
Chlorine . . .	24·74	19·0
	100·00	100·0

A substance having the composition specified in the second column, cannot be referred to any probable formula, but would appear to be composed of a mixture of silver with unaltered chloride, in about the proportion that would result from the destruction of two only in every seven parts of white chloride submitted to the action of the sun's rays. This, as already stated, represents the extent of decomposition on the *average* product; but special experiments, in which the nitrate of silver was employed in greater excess, and the chloride solution unusually dilute, enabled me to prepare, on the very brilliant day, June 16th, 1857, a product containing more than 82 per cent. of silver, and in which it might be assumed that one-third of the total amount of white chloride operated upon had become reduced to the metallic state. In attempting to pass this stage, the mechanical difficulty of the reduced silver particles encrusting and offering protection to the undecomposed white chloride, presents an obstacle which only extreme dilution and excessively fine state of division seem likely to combat.

In the application of the several chemical reagents by which it was proposed to eliminate the portion of unaltered chloride in admixture, and thus to isolate the essential matter which constituted the darker residual portion, it was found impossible to arrive at any other conclusion than that of its consisting of pure silver: throughout the examination no evidence was presented which pointed to the existence of the so-called subchloride of silver, or at least of its production under these circumstances; but, on the contrary, it appeared to be uniformly proved that *the metal,* somewhat modified in colour and physical condition by the varying circumstances of the experiment, was in every case the product resulting from the action of light upon the chloride of silver.

The reactions more particularly examined were the following:—

Ammonia dissolved out from the substance the whole of the unaltered chloride (afterwards recovered in a white scaly form by the evaporation of the solvent), while it left insoluble a grey residue of metallic silver in which no chlorine could be found.

Cyanide of potassium solution dissolved away the chloride and left metallic silver.

Hyposulphite of soda, employed in the form both of concentrated and dilute solutions, speedily dissolved out in the cold the unchanged chloride, leaving a residue of grey metallic silver, which contained neither oxygen, chlorine, nor sulphur.

Iodide of potassium operated first in the conversion of the chloride into yellow iodide of silver, which, on adding an excess of the reagent, was entirely taken into solution. The grey

metallic residue, after repeatedly washing with dilute iodide of potassium solution and finally with water, was found to consist of silver without any admixture either of chlorine or iodine.

Nitric acid in a cold and diluted form was inactive; but more concentrated acid effected the removal of the reduced silver by converting it into nitrate (with evolution of red nitrous fumes), and left insoluble the white chloride of silver; ammonia then added dissolved completely this latter substance.

On the other hand, the darkened product was reconverted into its original white condition, with varying degrees of rapidity, by treatment with chlorine-water, nitro-hydrochloric acid, the brown solution of bichloride of manganese, and by a mixture of hydrochloric acid and chlorate of potassa. An acid solution of the green chloride of copper had also the power of reconverting, although more slowly, the darkened chloride into its primitive condition; and a similar change appeared to be brought about by digesting in a cold saturated solution of chloride of mercury, but in this instance the conversion was attended with a reduction of the mercury salt to the state of subchloride, so that a black residue, derived from the calomel, remained on afterwards attempting to dissolve the chloride of silver in ammonia.

It was in the next place thought desirable to prepare for comparison a sample of altered chloride which had not been so fully acted upon by the light, and to restrict the excess of nitrate of silver employed, in order to ascertain whether at an earlier stage a more partial reduction, attended with the formation of an inferior chloride, could possibly occur. On a cloudy day in September 1857, a purple product was obtained, which differed from the former samples by containing a much larger proportion of unchanged chloride; and in consequence of the more marked physical change in the state of aggregation of the particles attending the removal of this larger quantity of unaltered matter, the colour of the substance exhibited a more striking transition from purple to grey on treatment with hyposulphite of soda and other solvents already enumerated. Neither in this instance was any chlorine detected in the insoluble residual portion, nor evidence furnished of its having been removed from a state of weak chemical combination.

It will be perceived that the results now recorded bear reference to a series of experiments from which the interfering influences of organic matter, and all other chemical agents, excepting only water and the nitrates of silver and soda, have been intentionally excluded. The motive for such a course rests on the belief that the full and accurate determination of the action of light *in its simplest phase* must precede other considerations likely to involve secondary and more complex reactions, which

will be better investigated after a full knowledge of the first has been acquired.

The fact of chlorine being evolved during the decomposition by solar agency of chloride of silver under water, has been repeatedly observed and is fully corroborated by my own experiments. It follows, therefore, that if a solution of nitrate of silver be employed in conjunction with the chloride, as in the ordinary practice of photography, the evolved chlorine will exert its own peculiar action on the silver solution in contact, precipitating from it an additional amount of white chloride, which in turn becomes, partially at least, decomposed by light. It has been assumed that *the whole* of the nascent chlorine is thus available for the formation of new chloride of silver; but this can, I think, be true only if other matters are present having the power to decompose hypochlorous acid, a product always formed to the amount of *half* that of the available chlorine, according to the reaction first pointed out by Balard,

$$Cl^2 + AgO, NO^5 = AgCl + Cl\,O + NO^5.$$

It might then be predicted that, by exposing pure white chloride of silver under a solution of the nitrate of known strength, this latter would become continually weakened. This I find to be the case; and in the event of employing a dilute solution, every trace of silver is removed, leaving only mixed nitric and hypochlorous acids as residual products dissolved in the water. It is possible also to remove the metal from a solution of nitrate of lead, by exposure to sunlight in contact with recently precipitated chloride of silver.

Again, inasmuch as the white chloride darkens with a rapidity regulated by the energy with which the liberated chlorine is removed from its sphere of influence, I have been able to prove* that reducing agents, the protochloride of tin especially, as also certain alkaline solutions, greatly facilitate the decomposition; while the higher chlorides of platinum and mercury are known to exert a power in the opposite direction.

Several experiments were also made upon the chloride of silver formed by the direct union of its elements—silver-leaf, electroplated daguerreotype tablets, and the silver specula obtained on collodionized glass by the ordinary photographic processes; these several conditions of silver, converted into chloride by the action of chlorine gas, furnished products all of which suffered decomposition on exposure to sunshine, but were very much less speedily affected than the condition of precipitated chloride

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* "On the alteration of Chloride of Silver by Light," Photographic News, October 1859.

usually employed in the practice of photography. The resulting darkened surfaces were found to be capable of restoration by renewed exposure to gaseous chlorine. These experiments were pursued no further, as they did not appear to present so close a parallel to the ordinary application of chloride of silver as the system of preparation already described.

From a general examination of the products obtained by the action of light upon several of the more definite compounds of silver, it appeared to me that the oxalate would be likely to present the most indubitable evidence of reduction *to the metallic state,* and from its ready decomposability be well fitted for comparison with the results afforded by the examination of the altered chloride. Some oxalate of silver was therefore prepared by precipitation from nitrate of silver and oxalate of ammonia (the former in slight excess), washed by decantation, and exposed under pure distilled water to the direct rays of the sun. The white oxalate soon changed colour, becoming reddish brown, and was seen to evolve small gas bubbles, which proved to be carbonic acid by the water having now the property of precipitating basic acetate of lead, producing a milkiness easily soluble in acetic acid. That *silver* was, on the other hand, the product of reduction, became evident on repeating the experiment with a more finely divided condition of the substance, and by removing the large proportion of unaltered material, which in this instance also remained in admixture with the blackened particles. The same transition of colour from dark purple to grey, attended the withdrawal of the undecomposed oxalate by dilute hyposulphite of soda solution, and the metal was left in a state of purity. Hence the decomposition will be expressed by

$$AgO, C^2O^3 = Ag + 2CO^2.$$
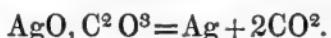
Passing in review the results obtained in the foregoing experiments, it will probably be considered that the weight of evidence tends to show that *the metal* is the ordinary product of the chemical action of light upon chloride of silver; and that the principal difficulty which has stood in the way of accepting this conclusion has in a great measure to be accounted for by the often varying shades of colour presented by the reduced metal, and more especially the transition observed at the moment of removing the unaltered portion of material by the application of the fixing agent. If in these several stages the change in *physical* condition be considered in its proper connexion, and due allowance be made for the very important influence known to be exercised over the light-reflecting capacity of these minutely divided particles by very slight modifications in their state of aggregation (quite irrespective of change in chemical constitu-

tion), there will then be no longer any difficulty in referring these results, with others of the same class (*e. g.* the several varieties of gold prepared and examined by Professor Faraday*), to a series all of which are capable of similar explanation.

I subjoin, in the form of propositions, a statement of the results arrived at ; they appear to me to have been fully substantiated by the foregoing experimental considerations. And I will remark, in conclusion, that the hypothesis believed to be supported by the facts now communicated is in conformity with the previous results of Dr. Guthrie, MM. Girard and Davanne, and generally also with those of M. Van Monkhoven, and will consequently be to a certain extent opposed to the views advanced by Messrs. Hadow, Hardwich, Llewellyn, and Maskelyne, in their joint report upon this subject recently presented to the meeting of the British Association.

Propositions.

1st. That chloride of silver, when decomposed by light, is separated into its elements.

2nd. That this change does not usually extend to the whole bulk of the material operated upon, on account of the opacity of the darkened product mechanically protecting a certain portion of unaltered chloride of silver from the action of the light.

3rd. That the degree and rapidity of reduction is influenced by the state of division of the particles, and by the presence of agents capable of absorbing the chlorine when liberated from its combination with silver.

Chemical Department, Royal Arsenal,
  Woolwich, February 17, 1860.

XXV. *On the Simultaneous Emission and Absorption of Rays of the same definite Refrangibility ; being a translation of a portion of a paper by* M. Léon Foucault, *and of a paper by* Professor Kirchhoff.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

SOME years ago M. Foucault mentioned to me in conversation a most remarkable phænomenon which he had observed in the course of some researches on the voltaic arc, but which,

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* Dr. Faraday showed that the *red* gold precipitated from solution by phosphorus became *violet* merely by the addition of chloride of sodium. (Notices of the Meetings of the Royal Institution, June 13, 1856.)

though published in *L'Institut*, does not seem to have attracted the attention which it deserves.   Having recently received from Prof. Kirchhoff a copy of a very important communication to the Academy of Sciences at Berlin, I take the liberty of sending you translations of the two, which I doubt not will prove highly interesting to many of your readers.

<div align="center">

I am, Gentlemen,

Yours sincerely,

G. G. STOKES.

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M. Foucault's discovery is mentioned in the course of a paper published in *L'Institut* of Feb. 7, 1849, having been brought forward at a meeting of the Philomathic Society on the 20th of January preceding.   In describing the result of a prismatic analysis of the voltaic arc formed between charcoal poles, M. Foucault writes as follows (p. 45) :—

" Its spectrum is marked, as is known, in its whole extent by a multitude of irregularly grouped luminous lines ; but among these may be remarked a double line situated at the boundary of the yellow and orange.   As this double line recalled by its form and situation the line D of the solar spectrum, I wished to try if it corresponded to it ; and in default of instruments for measuring the angles, I had recourse to a particular process.

" I caused an image of the sun, formed by a converging lens, to fall on the arc itself, which allowed me to observe at the same time the electric and the solar spectrum superposed ; I convinced myself in this way that the double bright line of the arc coincides exactly with the double dark line of the solar spectrum.

" This process of investigation furnished me matter for some unexpected observations.   It proved to me in the first instance the extreme transparency of the arc, which occasions only a faint shadow in the solar light.   It showed me that this arc, placed in the path of a beam of solar light, absorbs the rays D, so that the above-mentioned line D of the solar light is considerably strengthened when the two spectra are exactly superposed.   When, on the contrary, they jut out one beyond the other, the line D appears darker than usual in the solar light, and stands out bright in the electric spectrum, which allows one easily to judge of their perfect coincidence.   Thus the arc presents us with a medium which emits the rays D on its own account, and which at the same time absorbs them when they come from another quarter.

" To make the experiment in a manner still more decisive, I projected on the arc the reflected image of one of the charcoal points, which, like all solid bodies in ignition, gives no lines ; and under these circumstances the line D appeared to me as in the solar spectrum."

Professor Kirchhoff's communication "On Fraunhofer's Lines," dated Heidelberg, 20th of October, 1859, was brought before the Berlin Academy on the 27th of that month, and is printed in the *Monatsbericht*, p. 662.

"On the occasion of an examination of the spectra of coloured flames not yet published, conducted by Bunsen and myself in common, by which it has become possible for us to recognize the qualitative composition of complicated mixtures from the appearance of the spectrum of their blowpipe-flame, I made some observations which disclose an unexpected explanation of the origin of Fraunhofer's lines, and authorize conclusions therefrom respecting the material constitution of the atmosphere of the sun, and perhaps also of the brighter fixed stars.

"Fraunhofer had remarked that in the spectrum of the flame of a candle there appear two bright lines, which coincide with the two dark lines D of the solar spectrum. The same bright lines are obtained of greater intensity from a flame into which some common salt is put. I formed a solar spectrum by projection, and allowed the solar rays concerned, before they fell on the slit, to pass through a powerful salt-flame. If the sunlight were sufficiently reduced, there appeared in place of the two dark lines D two bright lines; if, on the other hand, its intensity surpassed a certain limit, the two dark lines D showed themselves in much greater distinctness than without the employment of the salt-flame.

"The spectrum of the Drummond light contains, as a general rule, the two bright lines of sodium, if the luminous spot of the cylinder of lime has not long been exposed to the white heat; if the cylinder remains unmoved these lines become weaker, and finally vanish altogether. If they have vanished, or only faintly appear, an alcohol flame into which salt has been put, and which is placed between the cylinder of lime and the slit, causes two dark lines of remarkable sharpness and fineness, which in that respect agree with the lines D of the solar spectrum, to show themselves in their stead. Thus the lines D of the solar spectrum are artificially evoked in a spectrum in which naturally they are not present.

"If chloride of lithium is brought into the flame of Bunsen's gas-lamp, the spectrum of the flame shows a very bright sharply defined line, which lies midway between Fraunhofer's lines B and C. If, now, solar rays of moderate intensity are allowed to fall through the flame on the slit, the line at the place pointed out is seen bright on a darker ground; but with greater strength of sunlight there appears in its place a dark line, which has quite the same character as Fraunhofer's lines. If the flame be taken away, the line disappears, as far as I have been able to see, completely.

" I conclude from these observations, that coloured flames in the spectra of which bright sharp lines present themselves, so weaken rays of the colour of these lines, when such rays pass through the flames, that in place of the bright lines dark ones appear as soon as there is brought behind the flame a source of light of sufficient intensity, in the spectrum of which these lines are otherwise wanting. I conclude further, that the dark lines of the solar spectrum which are not evoked by the atmosphere of the earth, exist in consequence of the presence, in the incandescent atmosphere of the sun, of those substances which in the spectrum of a flame produce bright lines at the same place. We may assume that the bright lines agreeing with D in the spectrum of a flame always arise from sodium contained in it ; the dark line D in the solar spectrum allows us, therefore, to conclude that there exists sodium in the sun's atmosphere. Brewster has found bright lines in the spectrum of the flame of saltpeter at the place of Fraunhofer's lines A, *a*, B ; these lines point to the existence of potassium in the sun's atmosphere. From my observation, according to which no dark line in the solar spectrum answers to the red line of lithium, it would follow with probability that in the atmosphere of the sun lithium is either absent, or is present in comparatively small quantity.

" The examination of the spectra of coloured flames has accordingly acquired a new and high interest ; I will carry it out in conjunction with Bunsen as far as our means allow. In connexion therewith we will investigate the weakening of rays of light in flames that has been established by my observations. In the course of the experiments which have at present been instituted by us in this direction, a fact has already shown itself which seems to us to be of great importance. The Drummond light requires, in order that the lines D should come out in it dark, a salt-flame of lower temperature. The flame of alcohol containing water is fitted for this, but the flame of Bunsen's gaslamp is not. With the latter the smallest mixture of common salt, as soon as it makes itself generally perceptible, causes the bright lines of sodium to show themselves. We reserve to ourselves to develope the consequences which may be connected with this fact."

Note.—The remarkable phænomenon discovered by Foucault, and rediscovered and extended by Kirchhoff, that a body may be at the same time a source of light giving out rays of a definite refrangibility, and an absorbing medium extinguishing rays of that same refrangibility which traverse it, seems readily to admit of a dynamical illustration borrowed from sound.

We know that a stretched string which on being struck gives out a certain note (suppose its fundamental note) is capable of being thrown into the same state of vibration by aërial vibrations corresponding to the same note. Suppose now a portion of space to contain a great number of such stretched strings, forming thus the analogue of a " medium." It is evident that such a medium on being agitated would give out the note above mentioned, while on the other hand, if that note were sounded in air at a distance, the incident vibrations would throw the strings into vibration, and consequently would themselves be gradually extinguished, since otherwise there would be a creation of *vis viva.* The optical application of this illustration is too obvious to need comment.—G. G. S.

XXVI. *Observations on the Theory of Equations of the Fifth Degree.* By James Cockle, *M.A., F.R.A.S., F.C.P.S. &c.**

[Concluded from vol. xviii. p. 510.]

75. A DAPTING the Eulerian or Bezoutian formulæ to the trinomial, and eliminating c and d, we find (compare art. 44, note)

$$a^3b^4 + \vartheta^2b^3 - Qa^2b^2 - \vartheta a^4b + \vartheta^3a = 0, \quad \cdots \quad (e')$$

$$\vartheta a^2b^6 + a^6b^4 - 3\vartheta^2a^3b^3 - \vartheta^4b^2 - \vartheta^3a^4 = 0, \quad \cdots \quad (f')$$

$$\left. \begin{array}{l} a^5b^{10} + 20\vartheta a^6b^7 + (a^{10} - 10Q\vartheta a^5 + Ea^5 + \vartheta^5)b^5 \\ \quad + 20\vartheta^4a^4b^3 - \vartheta^5a^5 = 0. \end{array} \right\} \quad \cdots \quad (g')$$

76. Form the equation

$$\left(-\frac{\vartheta}{a}b^2 + \frac{\vartheta^3}{a^4}b - a^3 - Q\frac{\vartheta}{a^2} - \frac{\vartheta^5}{a^7} \right)(e') + (f') = 0 ;$$

the result, cleared of fractions, is

$$\left. \begin{array}{l} -(3a^{10} + 2Q\vartheta a^5 + \vartheta^5)\vartheta^2b^3 + (Qa^{10} + Q^2\vartheta a^5 + Q\vartheta^5 - \vartheta^4a^5)a^2b^2 \\ \quad + (a^{10} - Q\vartheta a^5 + 2\vartheta^5)\vartheta a^4b - (2a^{10} + Q\vartheta a^5 + \vartheta^5)\vartheta^3a = 0. \end{array} \right\}$$

77. Form the equation

$$\left\{ \left(Q\frac{a^4}{\vartheta^3} + \frac{a^9}{\vartheta^4} + \frac{\vartheta}{a} \right)b^2 + \frac{a^6}{\vartheta^2}b + a^3 \right\}(e') + (f') = 0 ;$$

the result, cleared of fractions and divided by b^3, is

$$\left. \begin{array}{l} (a^{10} + Q\vartheta a^5 + 2\vartheta^5)a^3b^3 + (2a^{10} + Q\vartheta a^5 + \vartheta^5)^2b^2 \\ -(Qa^{10} + Q^2\vartheta a^5 + Q\vartheta^5 - 3\vartheta^4a^5)a^2b - (a^{10} + 2Q\vartheta a^5 + \vartheta^5)\vartheta a^4 = 0. \end{array} \right\}$$

78. The elimination of b between these results may be expressed by the determinant

$$\begin{vmatrix} \alpha, & \beta a^2, & \gamma a^4, \\ \beta a^2, & \delta a^4, & \epsilon a, \\ \gamma a^4, & \epsilon a, & \zeta a, \end{vmatrix}$$

in which $\alpha, \beta, \ldots, \zeta$ are functions of a^5 and of ϑ. This determinant is of the form $\chi(\vartheta, a^5)a^2$, and rejecting the factor a^2, the result of the elimination of b will be of the form

$$\chi(\vartheta, a^5) = 0.$$

79. A result of the same form will be obtained if we eliminate b between (g′) and (e′) or (f′); for Θ can only appear in the final results of elimination under the form Θ^5, otherwise we should be led to equations one side of which would have five times as many values as the other.

80. Further: the formulæ of Euler and Bezout are not affected by the binary interchange $\left(a\,d\right)\left(b\,c\right)$, and we obtain, at pleasure, four systems of relations, which, for brevity, I shall write

$$(a, b, \vartheta) = 0, \quad (d, c, \vartheta) = 0, \quad (c, a, -\vartheta) = 0, \quad (b, d, -\vartheta) = 0;$$

and these systems show that a and d are inseparably connected in the formulæ, and that the ultimate results will assume the form of quadratic equations. And such is the form which the equations in u and v (art. 44) indicate.

81. Let, then,

$$a^{10} - 2\lambda a^5 + \vartheta^5 = 0$$

denote the result of eliminating b, c, and d from the equations of art. 75. This result is equivalent to

$$\Theta^{10} - 2\mu\Theta^5 + 5^{10}\vartheta^5 = 0,$$

and, solving as for a quadratic, we find

$$\Theta^5 = \mu \pm \sqrt{\mu^2 - 5^{10}\vartheta^5},$$

or, as we may write it,

$$\Theta^5 = \mu \pm \sqrt{\nu}.$$

82. That λ and μ are rational functions of ϑ^5, follows from the consideration that

$$2\mu = (\Theta')^5 + (\Theta^{\mathrm{iv}})^5,$$

and consequently that μ and ϑ are " similar " functions. Hence we may express μ in terms of ϑ by the process appropriate to such functions, or we may adopt Lagrange's method of division.

But the preferable course will be to proceed by elimination as in the present* discussion.

83. Let μ' and ν' be the values which μ and ν take when $\mathrm{P}-\vartheta$ is substituted for ϑ. The indicated form for the root of a general quintic is

$$\mathrm{M}+\tfrac{1}{5}\Big\{i^{m}\sqrt[5]{\mu+\sqrt{\nu}}+i^{2m}\sqrt[5]{\mu'+\sqrt{\nu'}} \\ +i^{3m}\sqrt[5]{\mu-\sqrt{\nu}}+i^{4m}\sqrt[5]{\mu'-\sqrt{\nu'}}\Big\}.$$

84. This expression coincides in substance with that differently deduced by Mr. Jerrard in his 'Essay.' It embraces the second solvable form of Euler (*Novi Comm. Petr.* p. 96 *et seq.*), as it probably may be made to do that of Abel (posthumous theorems, *Crelle,* vol. v. p. 336). It embraces also the first soluble form of Euler as well as that of Demoivre, and the one that I have calculated by making one only of Lagrange's functions vanish (Diary for 1858).

85. The vanishing of this function is marked by $\vartheta=0$, and the roots of the form last mentioned are comprised in the expression†

$$i^{m}\sqrt[5]{(10\mathrm{B}-\mathrm{A}-5\mathrm{Q})\mathrm{P}-\mathrm{E}}$$
$$+i^{2m}\sqrt[5]{\dfrac{\mathrm{PA}}{2}+\dfrac{\mathrm{P}^{4}}{2\mathrm{A}}+\sqrt{\left(\dfrac{\mathrm{PA}}{2}+\dfrac{\mathrm{P}^{4}}{2\mathrm{A}}\right)^{2}-\mathrm{P}^{5}}}$$
$$+i^{3m}\sqrt[5]{\dfrac{\mathrm{PA}}{2}+\dfrac{\mathrm{P}^{4}}{2\mathrm{A}}-\sqrt{\left(\dfrac{\mathrm{PA}}{2}+\dfrac{\mathrm{P}^{4}}{2\mathrm{A}}\right)^{2}-\mathrm{P}^{5}}}.$$

86. Presumably ϑ_{4} is a rational function of ϑ_{1}, and indeed I

* The mode of elimination that I have found to be the most convenient in practice is Newton's, in which we annihilate extreme terms alternately. The process used in the text is a modification of Newton's, arrived at thus: Let X and Y be of the mth and nth degrees in the quantity to be eliminated, and let X_1 and X_2 be indeterminate expressions of the $(n-m)$th degree in the same quantity. Form the expressions

$$\mathrm{X}_1\mathrm{X}+\mathrm{Y} \quad\text{and}\quad \mathrm{X}_2\mathrm{X}+\mathrm{Y},$$

and assign the indeterminate coefficients so that the first $n-m+1$ terms of the former and the last $n-m+1$ terms of the latter shall vanish. If we make

$$\mathrm{Q}\vartheta-a^{5}=f,\quad \vartheta^{4}+\mathrm{Q}a^{5}=g,$$

the unmodified method of Newton gives the cubics

$$f^{2}a^{6}b^{3}-4\vartheta^{2}(fa^{5}+2\vartheta^{5})a^{3}b^{2}-2\vartheta^{5}gb+\vartheta^{2}(fg-4\vartheta^{4}a^{5})a^{2}=0,$$
$$(fg-4\vartheta^{4}a^{5})a^{4}b^{3}+2\vartheta fa^{11}b^{2}-4\vartheta^{3}(g\vartheta+2a^{10})a^{3}b-g^{2}\vartheta^{2}=0$$

in place of those in the text.

† A and B (which I call respectively v and u in the 'Diary') are known and rational, but complicated functions of the coefficients. Compare art. 21 of Mr. Harley's paper on Symmetric Products in the Manchester Memoirs, vol. xv.

have inferred that it is (arts. 58, 59). The presumption is now rebutted. But since ϑ^5 replaces ϑ in the formulæ, we have to inquire whether $\vartheta_4{}^5$ be a rational function of $\vartheta_1{}^5$. If it be, then, since every root of a rational equation is a rational function of its own square (for the equation may be written $x\phi(x^2)+\psi(x^2)=0$), we see that $\theta_4{}^5$ must be a rational function of $\theta_1{}^5$, and $\theta_4{}^{10}$ of $\theta_1{}^{10}$. Consequently each of the expressions

$$\theta_1{}^5\,\theta_4{}^5+\theta_2{}^5\,\theta_6{}^5+\theta_3{}^5\,\theta_5{}^5 \text{ and } \theta_1{}^{10}\,\theta_4{}^{10}+\theta_2{}^{10}\,\theta_6{}^{10}+\theta_3{}^{10}\,\theta_5{}^{10}$$

must be a symmetric (and indeed rational) function of the roots of the given quintic. Hence it is readily seen that the cubic whose roots are the above three values of $\theta_m{}^5\,\theta_n{}^5$ will have all its coefficients symmetric in x, and therefore invariable under all interchanges of the x's. It would follow that $\theta_m{}^5\,\theta_n{}^5$ has only three values; and that for some one value (at least) of r and s we have
$$\theta_r^5=\theta_s^5,$$
an inadmissible result.

87. The same difficulty presents itself in another shape. Since all functions of the above form are invariable under interchanges of the x's, the doctrine of similar functions shows that the second coefficient of the cubic could only be determined by the solution of a quintic, even if the first were known. But inasmuch as one of the most distinguished of writers on the theory of equations has recently repeated the expression of a belief, formed many years ago, that the general quintic is soluble by means of an Abelian sextic, I shall add a few words upon the point.

88. Let $\quad V_{1,\,4}=a\theta_1{}^5\,\theta_4{}^5+b\theta_1{}^4\theta_4{}^4+\ldots+e\theta_1\theta_4+f,$
then, as* we know,
$$V_{1,\,4}+V_{2,\,6}+V_{3,\,5}=r_1(x_5),$$
where r_1 denotes a rational function. Let the ratios of $a, b, \ldots, e$

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* Recurring to arts 14 *et seq.*, and grouping the θ's thus,

$$\{\theta,\,\theta(a\,d)\},\quad \{\theta(a\,b),\quad \theta(c\,d)\},\quad \{\theta(a\,c),\quad \theta(b\,d)\},$$

the omitted interchange $(b\,c)$ being equivalent to $(a\,d)$, and the interchanges in each of the other groups being complementary, let every single interchange be applied. The order of the groups will or may be changed, but the members of each group will be inseparable. Consequently no possible interchange can, save as to the order in which they are written, affect the groups. And since (see art. 15) the form of ϕ is arbitrary, and we may make
$$\phi=x_5+ax_1+bx_2+cx_3+dx_4,$$
we see that many of our conclusions are true, whatever be the values of a, b, c, and d. It is when we seek a symmetric product that those quantities become unreal fifth roots of unity, and that ϕ becomes one of the functions of Lagrange and Vandermonde.

to f be so assigned that x_5 may disappear from r_1, in other words that $r_1(x_5)$ may be a symmetric function, say $r_1(0)$, of x. We may form the cubic

$$V^3 - r_1(0)V^2 + r_2(x_5)V - r_3(x_5) = 0,$$

the roots of which will be the above three values of V.

89. If x_5 does not disappear from r_2 and r_3, the determination of those functions depends upon the solution of a quintic, and cannot be attained. If it does disappear, the cubic becomes

$$V^3 - r_1(0)V^2 + r_2(0)V - r_3(0) = 0.$$

90. In the latter case, since $r(0)$ is not affected by any interchange of the x's, of the fifteen values of $V_{q, r}$ three only will be distinct. But (art. 62) this involves the relation

$$V_{q, r} = V_{q, s},$$

which is equivalent, for some finite value of n, to

$$\theta_r^n = \theta_s^n.$$

91. But no such relation exists among the roots of the sextic in θ, and no such cubic can be formed the coefficients of which shall be symmetric functions of x; and since the Ξ of Mr. Jerrard and my θ are similar functions (*fonctions semblables*), I am constrained to conclude that the supposed cubic of that eminent algebraist cannot be formed, and that *the supposition that the general quintic is soluble by an Abelian sextic involves the untenable supposition that the sextic in θ has equal roots, or roots some integral powers of which are equal.*

92. In perfect accordance with this conclusion is that deducible from the symmetric product, $\pi(\theta)$, of the sextic in θ. We find, by substitution in the formula which I have already* given, that

$$\pi(\theta) = \tfrac{1}{4}(108Q^5 - E^3)^2(5^{10}E)^2,$$

and that when π vanishes and cubic radicals appear, the sextic and the given quintic have each equal roots.

93. So, too, although I have succeeded in obtaining unsymmetric functions of θ which are symmetric in x, and therefore known, the doctrine of similar functions shows that these known quantities can only be applied to the solution of the sextic through the medium of a quintic.

94. The β and α roots of the 15-ic in γ can be obtained, or at all events verified, by a process resembling that employed in art. 72 for the other roots. But

$$\beta_1 + \beta_3 + \beta_4 + \beta_5 = 2QE - 3Q^2x_2^2 + 2Ex_2^3$$

is the type of the formulæ of verification; and β and α are the

roots of a 10-ic equation, each root of which is a rational and symmetric function of two roots of the given quintic.

95. Again, we may (art. 56) express $\mathfrak{S}_4$ as a rational function of Θ_1; and if for a moment we write

$$\left.\begin{array}{c} d \\ a \end{array}\right\} = \left.\begin{array}{c} \frac{1}{3}\Theta_1{}' \\ \frac{1}{3}\Theta_1{}^{iv} \end{array}\right\} = \sqrt{\mathfrak{S}_1}\left(\cos\frac{m}{5} \pm \sqrt{-1}\,\sin\frac{m}{5}\right),$$

$$\left.\begin{array}{c} c \\ b \end{array}\right\} = \left.\begin{array}{c} \frac{1}{3}\Theta_1{}'' \\ \frac{1}{3}\Theta_1{}''' \end{array}\right\} = \sqrt{P-\mathfrak{S}_1}\left(\cos\frac{m'}{5} \pm \sqrt{-1}\,\sin\frac{m'}{5}\right),$$

then $\mathfrak{S}_4$ expressed in terms of $\mathfrak{S}_1$ is

$$\mathfrak{S}_4 = P + \mathfrak{S}_1\left(1 + 2\cos\frac{2m}{5}\right) - 2(P-\mathfrak{S}_1)\cos\frac{2m'}{5} \pm 4\theta_1\sin\frac{m}{5}\sin\frac{m'}{5};$$

and if we elevate each side of this equation to the fifth power, expand and eliminate m and m' by means of

$$\cos m = \frac{\mu}{\mathfrak{S}_1^{\frac{5}{2}}}, \qquad \cos m' = \frac{\mu'}{(P-\mathfrak{S}_1)^{\frac{5}{2}}},$$

we shall have one of the actual expressions on which the foregoing and (virtually) Mr. Jerrard's argument is founded. As to my own particular view (arts. 58, 59), I may add that if $\mathfrak{S}_4$ were a rational function of $\mathfrak{S}_1$, the roots of the quintic would contain no quintic surds unless (which there is no reason to suppose, though I once suspected it) the theory of Abelian sextics is imperfect. The error of Mr. Jerrard inheres, in my opinion, in his mode of comparing the equations (ab) and (ac) at pages 80 and 81 of his most valuable 'Essay.' His functions $_1\Xi$, $_2\Xi$, $_3\Xi$, and $_4\Xi$ in art. 104 are foreign to the question, mere instruments for eliminating radicalities. They lead to no other result than that to which the immediate comparison of (ac) and

$$\Xi - {}_0\Xi = 0$$

would conduct us, viz. an expression for Ξ into which $P_{f(\beta,\epsilon)}$ enters irrationally.

96. The theory sketched in these papers has been developed in pages* more appropriate than the present to the details of mathematical processes. I would suggest that x may be expressed as a rational function of γ, and μ as an irrational func-

tion of θ; for

$$(5^2\mathfrak{I})^2 - 5^2 \mathrm{P}(5^2\mathfrak{I}) = \theta.$$

The object of research will not be a finite algebraic solution; but I have ascertained (and it may be worth noticing) that the particular form

$$x^5 - 5\mathrm{Q}x^2 + 2\mathrm{Q}\sqrt[3]{\overline{\mathrm{Q}^2}} = 0$$

is soluble by radicals.

97. The present discussion, then, seems to me to establish the insufficiency of two proposed methods of solving equations of the fifth degree, or rather equations in general, and to add to the moral evidence of the impossibility of the solution. Perhaps the want of universal assent to the argument of Abel may in some degree be owing to the want of uniformity in the views taken by Abel himself, by Sir W. R. Hamilton, by M. Kronecker, and by Galois, and it is unquestionably desirable that that argument should be simplified. But I do not think that M. Wantzel's modification of it meets the desire. The formulæ in his second step should, I think, be replaced by

$$\phi(x_2, x_3, x_1, x_4 \ldots) = \alpha \ \phi(x_1, x_2, x_3, x_4 \ldots),$$
$$\phi(x_3, x_1, x_2, x_4 \ldots) = \alpha^\lambda \phi(x_2, x_3, x_1, x_4 \ldots),$$
$$\phi(x_1, x_2, x_3, x_4 \ldots) = \alpha^\mu \phi(x_3, x_1, x_2, x_4 \ldots),$$

the only inference from which is

$$\alpha^{1+\lambda+\mu} = 1, \text{ or } 1 + \lambda + \mu \equiv 0 \ (\mathrm{mod.} \ n).$$

That $n = 3$, $\lambda = 1$, $\mu = 1$ cannot, I think, be inferred without previously showing that the only prime power of an unsymmetric function which can have two values only is a cube, and we are once more remitted to the arguments of Abel and Sir W. R. Hamilton. W. Wantzel's third step, too[*], seems open to objection. Perhaps the impossibility of cubic radicals entering into the root may afford the basis of the desired simplification.

4 Pump Court, Temple,
    February 6, 1860.

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[*] The cyclical interchanges of five do not coincide with the cyclical interchanges of three; and we can only infer that

$$(\alpha^h y)^n = (\beta^k z)^p,$$

where the symbols on the right refer to the quinary, and those on the left to the ternary interchanges.

XXVII. *On the Equilibrium and Motion of Liquids in Porous Bodies. By* M. J. JAMIN [*].

THE function performed by vegetables, which consists in the raising of water through their tissues to their leaves, has never yet been explained. This effect, however, must either be due to the play of special organs analogous to the human heart, or it must be determined by the exercise of molecular forces and gravity in the ligneous body. If the first hypothesis were true, physiology would, in all probability, have detected at least the existence of the supposed organs; from its silence, therefore, we are led to conclude their non-existence. On the other hand, if the second hypothesis holds good, the question enters the domain of general physics, and may with justice be studied experimentally with a view of imitating artificially this function of vegetables.

Regarding the problem from the latter point of view, M. Jamin announces having arrived at a plausible solution. In this, his first communication, however, the author occupies himself solely with certain preliminary phænomena of capillarity in tubes and porous bodies; he proposes subsequently to apply the principles he here establishes and to describe an apparatus, exclusively composed of inorganic substances, which in its structure presents a striking analogy to that of vegetables, and which possesses the property of raising water, as trees do, to a height greater than that of the atmosphere between a moist soil, from which this water is continually drawn, and the artificial leaves where the water is continually evaporated. The conclusion he announces is that capillary forces suffice to explain the motion of the sap in vegetables.

A capillary tube being taken, one of its extremities is put in communication with a vacuum; by so doing a current of air is established within the tube from the atmosphere to the vacuum. If then the finger, covered with wet linen, be alternately pressed against, and lifted from the free extremity of the tube, the operation being frequently repeated at very short intervals, columns of liquid separated by bubbles of air will traverse the tube with a velocity which, from being very great at first, will diminish as the operation proceeds, and finally become zero. A chaplet whose beads are air and water is thus obtained, and the apparatus thus prepared is found to possess peculiar properties.

When a pressure is exerted at one extremity the nearest beads recede quickly, the following ones are less displaced, and the more distant ones remain unmoved. By doubling the pressure twice as many beads are put in motion; or, more generally, the

number of beads to which motion is imparted is proportional to the magnitude of the pressure applied. Consequently the opposite extremity of the column only begins to be displaced when the difference between the pressures acting at its two extremities reaches a limit proportional to the number of beads in the column; and if this number be increased indefinitely, the limit in question will also be indefinitely increased. In this manner a pressure of three atmospheres, acting incessantly for fifteen days at the extremity of a very fine tube containing a great number of beads, failed to produce the least visible displacement of the liquid.

Inversely, when a partial vacuum is produced at one end of the tube the nearest bubbles of air dilate greatly, the intermediate ones less, and those furthest distant remain unaffected so long as the diminution of pressure does not exceed a limit proportional to the number of bubbles or beads. To make the experiment, a very long tube containing a great number of beads may be cemented into the upper part of a barometer-tube. The mercury will then maintain precisely the same position as it would do if the tube were perfectly closed.

This experiment shows that the pressure exerted at one extremity diminishes abruptly by a constant quantity at each place where the continuity of the column of liquid is interrupted; and this fact may be easily explained.

For it is probable that the first effect of the pressure H' is to alter the form of the nearest bead of liquid, by hollowing out its anterior surface and increasing the radius of curvature of the meniscus which bounds its posterior surface. A portion, L, of the pressure being thus expended in the deformation of the first bead, a deformation which cannot exceed a certain limit, and which is the same for all the beads, the residual pressure $H'-L$ is transmitted by it to the next succeeding air-bubble, and thus to the second bead, which, in becoming similarly deformed, again diminishes the pressure by the same amount as before. This action continues until the originally applied pressure has, by n equal, successive decrements, one at each bead, become reduced to $H'-nL=H$, the normal pressure in the tube, when, of course, equilibrium results.

By generalizing this idea, it is easy to show that the chaplet may assume an infinite number of states of equilibrium, whose conditions may be calculated; and experiment is found to verify the results of calculation.

It will be at once seen that these properties must considerably modify the ascent of liquids in capillary tubes. There are in fact two cases to be distinguished.

First. After raising the tube in the liquid in which one end is

immersed, and allowing the column of raised liquid to descend to its position of equilibrium, the length of the raised column is $a + n\mathrm{L}$, and consequently greater the greater the number n of interruptions ; this length may increase indefinitely.

In the case where the weight of each bead is equal to L, they are individually in equilibrium, and a column of indefinite height, interrupted only by very small bubbles, and everywhere at the atmospheric pressure, may be sustained.

Secondly. When the tube is depressed in the bath, and the liquid allowed to rise to its position of equilibrium, the length of the raised column is diminished in proportion to the number of bubbles, and becomes $a - n\mathrm{L}$; it is always less than if the column were continuous, and it may become negative and decrease indefinitely.

In accordance with theory, experiment also proves that when once a column of liquid, whose length is between the limits $a + n\mathrm{L}$ and $a - n\mathrm{L}$, has been placed in the tube it will remain there. Some experiments were also made with a view of measuring the limit L of the resistance which a single bead can oppose to the pressure. It was found that this limit is independent of the length of the bead, but that it increases when the bubbles of air diminish ; it increases, too, very rapidly when the diameter of the tube is diminished, and is equivalent to 54 millims. in a tube where the capillary ascension amounts to 200 millims. In such a tube, therefore, four interruptions are equivalent to the capillary force, and may annul the latter when the liquid rises, or double the height of the sustained column when the liquid descends. Mercury produces effects much more intense, but alcohol and oil oppose no resistance to pressure.

When a capillary tube, instead of being cylindrical, possesses successive contractions and expansions, it exhibits still more curious properties. After being once moistened, the thin film of liquid which remains adhering to its walls soon collects at the contractions, and there forms interrupted beads. Here then a chaplet is formed, as it were, spontaneously, and in consequence of the nature of the canal, the above-mentioned properties of a cylindrical tube become exaggerated in a surprising manner. A tube with eight very narrow contractions sufficed to close a barometer-tube hermetically, and even to overcome a pressure of two atmospheres.

If pressure be applied to one extremity of such a tube, filled with water, the latter overflows (*filtre*) without difficulty ; but if this pressure is exerted on a compressed gas, the latter replaces the water in each successive chamber, and leaves a bead of liquid at each contraction ; these beads, by opposing a resistance which increases with their number, finally destroy the pressure.

Inversely, when the tube is full of air and a column of water is forced into it by pressure, it fills the several chambers successively, destroys the beads, and annuls their resistance; finally, it fills the whole tube and commences to overflow.

These consequences may be applied to porous bodies in which we may assume the existence of canals alternately narrow and broad. When a porous vessel, such as is used in the battery, or an alcarraza, or a plaster statuette, or any other cavity formed in a porous mass is filled with water, any pressure exerted upon this water causes the same to filter through the mass; on the other hand, however, a perfect vacuum may be formed in the interior, atmospheric air being incapable of penetrating through the walls when moistened.

When both surfaces are immersed in water, and a pressure is exerted in the interior by means of compressed air, the latter, in the first place, expels all the water; but when this is accomplished, the air does not filter through the walls: the pressure, indeed, may be increased to two, three, and in some cases even to four atmospheres without causing the least air-bubble to traverse the porous sides; and this pressure, too, may be maintained for an indefinite period, exactly as if the sides of the vessel were not traversed by capillary fissures.

XXVIII. *Chemical Notices from Foreign Journals.* By E. ATKINSON, *Ph.D., F.C.S., Teacher of Physical Science in Cheltenham College.*

[Continued from p. 126.]

IN continuation of previous researches, Deville and Troost* have made some determinations of the specific gravities of certain vapours at high temperatures. The vessels used were porcelain flasks with narrow necks, of 280 cubic centims. capacity. These flasks are loosely closed by means of a small porcelain cylinder of 1 or 2 millims. diameter, which fits in the neck. At the termination of the experiment, the projecting end of this cylinder is fused by the oxyhydrogen lamp and closed effectually. The bath employed was that previously described, in which high constant temperatures are obtained by means of metallic vapours. In these experiments, Deville and Troost used cadmium vapour, the temperature of which is 860° C., and zinc vapour, the temperature of which is 1040° C.

The determinations were made by taking the density of the substance operated upon, and that of iodine under the same circumstances. Deville and Troost obtained in this manner a

very accurate relation between the densities of the two vapours, the density of one of which is perfectly well established.

The results arrived at were as follows :—

Sulphur.—The vapour-density of this substance at 860° is 2·2. For the temperature 1040° the same number was also obtained in twelve successive experiments. Previous determinations of other experimenters had furnished the number 6·6, which gave for sulphur an atomic value $\frac{1}{3}$ that of oxygen, phosphorus, &c. Various explanations have been offered to explain this anomaly, which is removed by Deville and Troost's observations.

Selenium.—Its vapour-density at 860° is 8·2 ; at 1040° it is 6·37. Theory and analogy with sulphur require the number 5·44. By making the determination at 12° to 1400° C., the authors hope to obtain this number.

Phosphorus.—Vapour-density at 1040°, 4·8. Calculated, 4·4.

Cadmium.—At 1040° the vapour-density is 3·94. For a condensation to two volumes the number 3·87 is required.

Chloride of Ammonium.—At 1040° the observed vapour-density was 1·01. For a condensation to eight volumes the number 3·87 is required.

Bromide of Aluminium.—Vapour-density 18·62. Theory requires 18·51.

Iodide of Aluminium.—The observed vapour-density was 27·0. The number required by theory is 27·8.

Bineau has also published* some determinations of the densities of superheated vapours, executed upwards of ten years ago. The experiments were made in tubes placed in a sort of cylindrical iron case, from which the finely drawn out extremities of the tubes projected. The capacity of each tube was determined before the experiment. The tubes were sealed by means of an oxyhydrogen flame. The tubes were covered with an argillaceous lute, and were surrounded by sand, or iron filings. The cylinder was placed horizontally on a grate, and heated as regularly as possible ; the temperature was determined by means of an air-thermometer ; and in order to test the method, a control determination of the density of mercury was made. It gave the number 6·7, which agrees well with 6·97, the number obtained by Dumas, and 6·91, the calculated number. A control determination of the density of iodine gave the number 8·65. Dumas obtained 8·716, and the calculated number is 8·8.

The numbers obtained for the density of sulphur vapour varied in nine experiments between 2·1 and 2·8. These experiments may be divided into two groups ; in one of these the temperature was below 800°, and the numbers were—

* *Comptes Rendus,* vol. xlix. p. 799.

Vapour-density.	Approximate temperature.
2·8	714°
2·7	727
2·6	731
2·8	743

In five experiments, where the temperature exceeded 800°, the results were—

Vapour-density.	Approximate temperature.
2·4	834°
2·6	851
2·4	963
2·1	1082
2·3	1162

At 450° to 500°, Dumas obtained the number 6·56, and Mitscherlich 6·9. Bineau assigns to sulphur vapour at 600° the density 5; at 700°, 2·8; and at 800°–1000° the density 2·2, which is the number obtained by Deville and Troost.

Berthelot has published* a detailed account of a series of experiments, preliminary notices of which have already appeared, in which he shows that cholesterine, meconine, and Borneo camphor belong to the class of alcohols. He considers an alcohol to be a neutral substance, consisting of carbon, hydrogen, and oxygen, which unites directly with acids, under elimination of water, to form a neutral compound, which by assimilation of the elements of water is again resolved into the substances of which it was composed.

The neutral ethers of the above substances are obtained by heating them with acids, in sealed tubes, for several hours, to a temperature of 200°, and then purifying the resultant substance by appropriate methods.

Cholesterine is considered by Berthelot to have the formula $C^{52} H^{44} O^2$, and to belong to the series of alcohols whose general formula is $C^n H^{n-8} O^2$, and to which cinnamic alcohol, $C^{18} H^{10} O^2$, belongs. Cholesterine forms compounds with stearic, butyric, and acetic acids, as also with hydrochloric and benzoic acids. The stearic acid compound, $C^{52} H^{43} O, C^{36} H^{35} O^3$, is a white body crystallizing in small brilliant needles. It melts at 65° to a colourless liquid which solidifies to a dull, uncrystalline waxy mass.

With spermaceti, which has long been recognized as an alcohol, Berthelot has prepared the stearic, butyric, acetic, and benzoic compounds. With the exception of the benzoate, they are difficult to obtain pure.

* *Annales de Chimie et de Physique,* vol. lvi. p. 51.

Meconine, $C^{20} H^{10} O^8$, also forms an ether when heated with stearic acid. It is a neutral, solid, colourless substance, and is readily fusible. It has the formula $C^{92} H^{78} O^{12}$; and its formation may be thus expressed:

$$C^{20} H^{10} O^8 + 2 C^{36} H^{36} O^4 - 4 HO = C^{92} H^{78} O^{12}.$$

Meconine.   Stearic acid.   New body.

Meconine appears to be a biatomic alcohol; and Berthelot considers that it stands in the same relation to its oxidation products, opianic acid and hemipinic acid, as ethylene does to aldehyde and acetic acid.

$C^{20} H^{10} O^8$	$C^4 H^4$
Meconine.	Ethylene.
$C^{20} H^{10} O^{10}$	$C^4 H^4 O^2$
Opianic acid.	Aldehyde.
$C^{20} H^{10} O^{12}$	$C^4 H^4 O^4$
Hemipinic acid.	Acetic acid.

Borneo camphor, $C^{20} H^{18} O^2$, according to Berthelot, stands in the same relation to ordinary camphor, $C^{20} H^{16} O^2$, as benzylic alcohol, $C^{14} H^8 O^4$, does to hydride of benzoyle, $C^{14} H^6 O^2$. Ordinary camphor is an aldehyde; it does not combine with acids; when it is heated with potash in a sealed tube for a long time at the temperature 180°, it is resolved, though with difficulty, into borneole (Borneo camphor) and a new acid, *camphic acid,* which probably has the formula $C^{20} H^{18} O^4$. The decomposition would be thus expressed:

$$2 C^{20} H^{16} O^2 + 2 HO = C^{20} H^{18} O^2 + C^{20} H^{16} O^4.$$
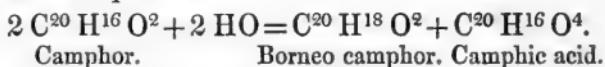
Camphor.   Borneo camphor. Camphic acid.

The artificial Borneo camphor, or *camphole,* has all the properties of the natural substance, excepting that it deviates the plane of polarization more strongly. Camphole combines with hydrochloric acid at 100° and with organic acids at about 200°. The stearate is a neutral, viscous, colourless, inodorous oil which sometimes crystallizes. The hydrochlorate, $C^{20} H^{17} Cl$, has all the physical properties of the substance isomeric with it, produced by the combination of hydrochloric acid with oil of turpentine, and known as " artificial camphor." Unlike this compound, however, the hydrochlorate of camphole is readily attacked by alcoholic alkalies. Camphic acid is very difficult to purify. Its formula is probably $C^{20} H^{16} O^4$; it is obtained as an almost solid colourless mass, heavier than water, in which it is insoluble. In the free state it is decomposed by heat. It is acted upon by nitric acid with formation of a nitro-compound. Camphate of soda produces precipitates in most of the metallic solutions.

Berthelot also examined the action of organic acids at a high temperature on orcine, thymole, alizarine, and the hydrates of oil of turpentine. All these substances, with the exception of orcine, contain 20 equivs. of carbon. Orcine, $C^{14} H^8 O^4$, appears to form a compound with stearic acid. The experiments with the other substances led to no positive results.

Vulpic acid, one of the lichen acids first isolated, and found in the *Cetraria vulpina,* was so little known that its identity as a distinct acid was doubted. Möller and Strecker* have recently investigated it, and have arrived at some very interesting results. The acid is prepared by macerating the lichen with lukewarm water containing milk of lime, and supersaturating the extract with hydrochloric acid; a precipitate is formed which is washed with cold water and dissolved in boiling alcohol. On cooling, the acid crystallizes in tolerably long needles of the monoclinic system, which in colour and lustre resemble sulphur. Its best solvent is chloroform, by which it may be directly extracted from the lichen. Its formula is $C^{38} H^{14} O^{10}$, and it is monobasic. It forms crystallized salts with potash, ammonia, lime, and baryta. The salts of the heavier metals are obtained by the double decomposition of vulpate of potash with the corresponding metallic solutions. In appearance vulpic acid most resembles usnic acid; and their composition only differs by the elements of water, the formula of usnic acid being $C^{38} H^{18} O^{14}$; but in their chemical deportments there is considerable divergence.

When vulpic acid is boiled in a distillation apparatus with baryta water, oxalate of baryta is gradually deposited, and a liquid distils over with the water, which is methylic alcohol. In the residual liquor from which the oxalate has precipitated, the soluble baryta-salt of a new acid is contained. It is obtained thus : the excess of baryta is removed by carbonic acid, the liquor filtered, concentrated, and supersaturated with hydrochloric acid. A crystalline substance separates, which, when recrystallized from alcohol, forms beautiful thin rhombic plates, which melt at $76°·5$ and distil at $265°$. This substance is strongly acid; it forms salts with alkalies and alkaline earths, which, however, from their great solubility, are difficult to crystallize.

The composition of the acid is $C^{16} H^8 O^4$, which is that of the toluylic acid obtained by Noad by oxidizing cymole by nitric acid. But the two substances are only isomeric, and not identical; for Noad's acid crystallizes in needles, and melts above $100°$. This new acid, which has been named *alphatoluylic acid,* agrees closely in its properties with the toluylic acid obtained by Cannizaro by boiling cyanide of benzyle with potash.

* Liebig's *Annalen,* January 1860.

Alphatoluylic acid is oxidized, but with difficulty, with formation of hydride of benzoyle. Strecker and Möller have prepared the chloride, the amide, and the nitro-compound of this acid. They represent its formation from vulpic acid in the following manner:—

$$\left.\begin{array}{l} \text{Vulpic acid. } C^{38}\, H^{14}\, O^{10} \\ +\,8\,\text{eq.water} \quad \underline{H^8 \quad O^8} \\ \hphantom{+\,8\,\text{eq.water} \quad} C^{38}\, H^{22}\, O^{18} \end{array}\right\} = \begin{array}{l} 2\,\text{eq. Alphatoluylic acid} \quad C^{32}\, H^{16}\, O^8 \\ \text{Oxalic acid } \quad . \quad C^4\, H^2\, O^8 \\ \text{Methylic alcohol. } \underline{C^2\, H^4\, O^2} \\ \hphantom{\text{Methylic alcohol. }} C^{38}\, H^{22}\, O^{18} \end{array}$$
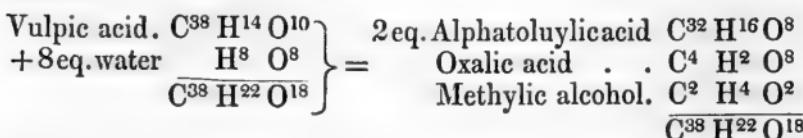
If, in the decomposition of vulpic acid, potash be used instead of baryta, other products occur. Methylic alcohol distils over; and when the residue is supersaturated with hydrochloric acid, carbonic acid is disengaged in abundance and a crystalline precipitate formed. This is a new acid, which, when recrystallized from boiling alcohol, appears in the form of rhombic prisms. The crystals are brittle, but tolerably hard; they melt at 154°; they are difficultly soluble in water, but readily so in alcohol. The formula of this acid is $C^{32}\, H^{16}\, O^6$; and the authors name it *oxatoluylic acid*. The baryta-, silver-, and lead-salts are all crystallized, and are monobasic. Oxatoluylic ether, $C^{32}\, H^{15}\, (C^4\, H^5)\, O^6$, is best obtained by the action of iodide of ethyle on the silver-salt. It forms colourless prismatic crystals, which melt at 45°·5. By the action of nitric acid on the acid, a nitro-compound is formed. The formation of oxatoluylic acid is as follows:—

$$\left.\begin{array}{l} \text{Vulpic acid } . \quad C^{38}\, H^{14}\, O^{10} \\ \text{Water } \quad . \quad . \quad \underline{H^6 \quad O^6} \\ \hphantom{\text{Water } \quad . \quad . \quad} C^{38}\, H^{20}\, O^{16} \end{array}\right\} = \begin{array}{l} C^{32}\, H^{16}\, O^6 \quad \text{Oxatoluylic acid.} \\ C^2\, H^4\, O^2 \quad \text{Methylic alcohol.} \\ \underline{C^4 \hphantom{\, H^{16}} O^8} \quad \text{Carbonic acid.} \\ C^{38}\, H^{20}\, O^{16} \end{array}$$
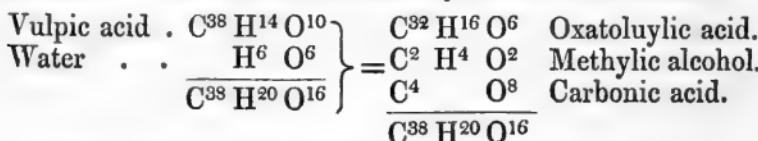
When boiled with strong potash, oxatoluylic acid readily undergoes a further change; it becomes converted into oxalic acid and toluole. From this deportment it appears to have received its name. The decomposition may be thus expressed:—

$$C^{32}\, H^{16}\, O^6 + 2\,HO = C^4\, H^2\, O^8 + 2\,C^{14}\, H^8.$$
<div style="text-align:center">Oxatoluylic acid.   Oxalic acid.   Toluole.</div>
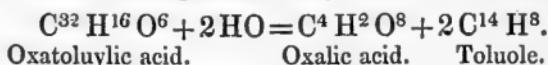
The difference in the decomposition of vulpic acid by potash and by baryta is remarkable as showing a great mobility of the atoms. It appears probable that the insolubility of oxalate of baryta is the determining moment in the first reaction, as is frequently observed in double decompositions of salts.

Salicylic acid, $C^{14}\, H^6\, O^6$, has been considered by Kolbe to have a constitution analogous to that of carbovinic acid; in short, to be carbophenylic acid,—

Carbovinic acid . . $\left.\begin{array}{c} C^4 H^5 O \\ H \quad O \end{array}\right\} C^2 O^4.$

Carbophenylic acid . $\left.\begin{array}{c} C^{12} H^5 O \\ H \quad O \end{array}\right\} C^2 O^4.$
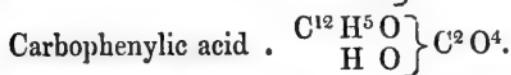
This view is supported by the well-known fact that salicylic acid decomposes, when heated with powdered glass, into phenylic alcohol and carbonic acid.

Kolbe and Lautemann[*] have recently effected the synthesis of this acid from phenylic alcohol and carbonic acid. It is not formed when carbonic acid is passed into sodium-phenylic alcohol, $C^{12} H^5 Na O^2$; but when carbonic acid is passed into phenylic alcohol while sodium is being dissolved in it, the three bodies unite with liberation of hydrogen to form salicylate of soda. On neutralizing this product with hydrochloric acid, and boiling with water to expel excess of phenylic alcohol, salicylic acid pure and in tolerable quantity is obtained.

By analogous methods these chemists hope to obtain from hydrate of cresyle and hydrate of thymole the corresponding acids, and from bisulphide of carbon and hydrated oxide of phenyle the compound

$$\left.\begin{array}{c} C^{12} H^5 O \\ H \quad O \end{array}\right\} C^2 S^4 \text{ (Phenylxanthogenic acid).}$$
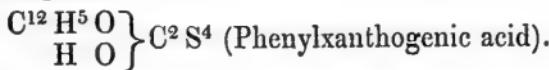
In some recent experiments with flesh, Scherer[†] has found that xanthine[‡] is contained in muscle, and also in the pancreas. In the former it exists along with hypoxanthine, which he has shown to be identical with Strecker's sarcine; in the pancreas it exists along with guanine. The pancreas contains about 0·0166 per cent. of xanthine, and 0·01223 of guanine. It is accordingly a better source for xanthine than flesh, which only contains 0·003 per cent.

Scherer has also observed that leucine is contained in the pancreas, and in such large quantities that the latter is the most valuable source for it. The following experiments will show how it is obtained therefrom. Twenty pounds of finely chopped pancreas were boiled in water for about five minutes, the mixture filtered, and the residue treated with hot water. The filtrate was precipitated by baryta water, filtered, and the filtrate evaporated on the water-bath with the addition of acetate of copper. The precipitate formed, which consisted of xanthine and guanine in combination with copper, was filtered off. On saturating this filtrate with sulphuretted hydrogen, and evaporation, the liquor yielded about six ounces of pure leucine containing mere traces

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[*] Liebig's *Annalen*, January 1860.　　　　[†] Ibid. December 1859.
[‡] Phil. Mag. vol. xviii. p. 135.

of tyrosine. This quantity corresponds to 1·77 per cent. of the fresh pancreas, or, since that contains 76 to 77 per cent. of water, to 7·37 per cent of the solid constituents. A subsequent experiment, made with a fresh pancreas under special precautions, showed that leucine pre-existed in the gland, and was not a product of decomposition.

Professor Schmidt and Dr. Stürzwage in Dorpat have made a series of experiments on the action of arsenious acid, when introduced into the circulation, on the oxidizing process in the body. The mode of experimenting consisted in determining the normal quantity of carbonic acid exhaled in an hour by certain animals (fowls, pigeons, and cats), and then administering to them arsenious acid, and again observing the quantity of gas exhaled in the same time. The apparatus consisted of a bell-jar, standing on a ground-glass plate, under which the animal was placed. In the tubulure of the bell-jar were inserted two tubes, and a delicate thermometer. One of these tubes communicated freely with the air, the other was connected with a series of tubes for the absorption of carbonic acid and water, and with an aspirator by which a regulated quantity of air could be drawn through the system. Each experiment lasted about an hour, during which time about 30 to 35 litres of air were drawn through : the carbonic acid of this air was determined by a separate experiment, and allowed for. The secretion of urea was determined in some cases : the determinations were made by Liebig's method.

A fowl weighing 896 grms. was found to respire on an average 2·07 grms. CO^2 in the hour. A solution of 0·018 grm. arsenious acid was then introduced into its crop, and after half an hour the bird was placed under the jar. It was found to respire 1·88 grm. of CO^2 in the hour. On the next day 0·027 grm. of AsO^3 were administered; the quantity of CO^2 diminished to 1·35 grm.; at the same time the bird was attacked by severe diarrhœa, its respiration became greatly accelerated, it drank much water, and trembled violently. On the following day these symptoms diminished, and 0·035 grm. AsO^3 were injected. Three hours after, the expiration of carbonic acid diminished to 1·296 grm.

In an experiment with another fowl, the injection of 0·032 grm. AsO^3 caused the respiration to diminish from 2·085 grms. in the hour to 1·75 grm.

In another fowl weighing 1400 grms., the respiration of CO^2 was 2·37. An hour and a half after the injection of 0·035 AsO^3 it fell to 1·92 grm., and ten hours after to 1·37 grm. It showed at the same time the usual symptoms of arsenical poisoning, but afterwards gradually recovered, and in five days its respiration

had reached the normal quantity. On administering the previous dose, the quantity diminished to 1·27 grm.

With a cat weighing 2·61 kilogs., and which consumed daily 130 grms. of flesh, the normal respiration was 3·08 grms. 0·025 grm. AsO^3 were injected into the jugular vein ; the quantity fell to 2·301 grms., and subsequently the animal died.

With another cat the average respiration of carbonic acid in an hour was 2·925 grms., and the average amount of urea secreted per diem was 9·85 grms. After the administration of 9·01 grms. AsO^3, the respiration sank to 1·98 grm., and the secretion of urea to 3·61 grms.

With starving animals, previous investigations (by Bidder and Schmidt) showed that the respiration diminishes in the first forty-eight hours, but afterwards remains constant even until the sixteenth day, notwithstanding the continuous decrease in weight of the animal. To ascertain the effect of arsenious acid upon starving animals, the following experiments were made. A cat whose respiration amounted to 3·3 grms., was left without food for three days. On the first day the respiration had sunk to 2·45, and on the third to 2·24 grms. 0·018 grm. As O^3 were then injected into the jugular vein. The respiration of CO^2 decreased to 1·902 grm. The action of the heart also diminished greatly.

In another experiment with a cat weighing 3·31 kilogs., and fed on 150 grms. flesh daily, the respiration and the secretion of urea were determined for four days. The animal was then deprived of food, and the same determinations made for the same time. The weight and the secretion of urea remained constant for the first four days; during the next four days the weight diminished to 2·88 kilogs., and the respiration of carbonic acid from 3·45 grms. to 2·54 grms. An injection of 0·005 grm. As O^3 into the jugular vein was then made, and the animal fed with meat, which it consumed with great avidity, but afterwards vomited all but 24 grms. On the following day it refused food. The respiration of CO^2 diminished to 2·24 grms., while the bodily weight slightly increased, and underwent no diminution until the third day after the injection, during which time the animal rejected all nourishment. This result is the more surprising since, in the earlier period of inanition, fasting for twenty-four hours produced a considerable diminution in the bodily weight.

From these experiments, Schmidt and Stürzwage conclude that arsenious acid introduced into the organism occasions a considerable diminution in the secretion of matter. The phænomena are most observable in fowls; but even in cats, which vomit after the injection, and are to be considered as starving, the diminution amounts to 20 per cent., even after eliminating

the diminution caused by mere inanition. This fact explains the fattening of horses after the administration of small doses of arsenious acid, a fact well known to horse-dealers. That quantity of fat, and of albumen, which corresponds to the depression in the secretion of carbonic acid and urea, remains in the body; and if the animal receive adequate nourishment, its weight increases.

XXIX. *Instructions for the better observation of the Scintillation of the Stars. By* CHARLES DUFOUR, *Professor of Mathematics at Morges* *.

UNTIL lately the study of the scintillation of the stars has not formed the subject of any series of observations. Here and there may be found a few isolated directions, and several persons have attempted divers explanations of the phænomenon, but no continued observations have as yet been presented to the learned world. I believe I am the first who has undertaken a work of this kind. My observations, commenced at Morges in 1852, were at first but a series of gropings in the dark, but since 1853 down to the present time, I have never allowed one evening to pass in which the stars were visible, without carefully observing the scintillation; and after six years' perseverance in the work, I am convinced that this branch of astronomical study is important, and merits a place amongst meteorological observations.

But in order that the results obtained may be more general and more complete, it would be very desirable to undertake a series of observations analogous to those I have commenced, in other climates and under other meteorological circumstances.

At the time I am writing (December 1859) the following are the stations where, I hope, a work of this kind is begun or continued.

Morges (Switzerland), 46° 30″ North latitude and 4° 9″ longitude east of Paris.—Since 1853 I have taken at Morges nearly 24,000 observations on the scintillation of the stars. The principal results obtained up to the present time, have been published in the reports issued by the Academies of Belgium and Paris, the 'Notices' of the Astronomical Society of London, and the *Bulletin de la Société Vaudoise des Sciences Naturelles.*

The Great St. Bernard, at an altitude of 2480 metres.—The monks who inhabit this elevated spot all the year round, on the borders of the eternal snow, have willingly undertaken to carry on the observations that I commenced there in the summer of

1856, during the time I was staying at the hospital for scientific purposes.

Grand Cairo, where Mahmoud Effendi, Director of the Observatory there, has already commenced, or will immediately commence, a series of observations analogous to my own at Morges.

The Peak of Teneriffe, where Mr. Piazzi Smyth, Director of the Observatory of Edinburgh, has already passed several months for scientific purposes. This gentleman has promised me, that if, as he hopes, he is able to continue his researches on this isolated mountain, he will give special attention to the study of the scintillation of the stars.

Several localities in Russia.—Last year the Geographical Society of St. Petersburgh decided on publishing instructions for the observation of all kinds of meteorological phænomena, and M. Kaëmtz, who was charged with the direction of the publication, requested me to draw up the part relating to the scintillation; so that I have reason to believe that in various parts of Russia, observations in accordance with these instructions have already been commenced.

Havanna, where M. Poey has founded an observatory during the past year. This gentleman, with a view to observing the scintillation at Havanna after my method, requested me to forward him a copy of the instructions already sent to Russia; so that there also, I hope, the work is begun. But these stations are very wide apart on the surface of the globe.

There are certain countries and certain climates from which it is most desirable that observations should be taken; for instance, the Torrid Zone, as the observations taken at Havanna are the only ones within the tropics. It would be well to have several stations in various parts, as it is important to know what is the amount of scintillation in the hottest and dampest countries in the world. In the Austral Hemisphere also, observations would be extremely valuable, because there not only would the student have the opportunity of observing stars invisible to us (Acherner and Campus among others), but the meteorological phænomena, being notably different, would doubtless sensibly affect the scintillation. Those beautiful stars of our own hemisphere also, Sirius and Rigel, which we see only in winter, are visible south of the equator during the hot season, and are also nearer the zenith; and it would be interesting to know if their scintillation is at all affected by these circumstances.

In countries situated to the extreme north, it would also be extremely interesting and important to procure observations, and to ascertain the amount of scintillation in those serene nights of intense frost so constant in Eastern Siberia and even in European Russia.

If it were possible to procure observations from the Polar regions during their long night of several months, it would be specially interesting ; and I would take the present opportunity of recommending this branch of study to the notice of travellers who may be passing one or more winters in the midst of the ice of a Polar region. I regret exceedingly not having begun my observations ten years sooner, as then those hardy mariners, who, in search of Sir John Franklin, spent so many winters and travelled over so great a portion of those regions, might perhaps have been induced to devote some of their time to this branch of study.   But similar expeditions may be undertaken again ; and if such should be the case, I promise myself to call the attention of future explorers to this phænomenon, in the hope of procuring valuable results from their observations.

But in order to facilitate their efforts, and to avoid useless expenditure of time, as well as to render the indications given by one observer capable of being compared with the observations made by another, I would recommend the use of the following instructions, as the fruits of the experience acquired in the long series of observations taken by me at Morges.

1st. *The manner of observing.*

I have tried several scintillometers—all those indicated by Arago, and a new one proposed by myself ; but I am convinced that none of them are worth the observations taken by the naked eye.   After a little practice it becomes easy to ascertain with tolerable exactitude whether a star scintillates more or less than another star, and to indicate the amount of scintillation by a given number, in like manner as in meteorology the state of the sky, or the force of the wind, is indicated by figures.   My own method is to designate by 0 the absence of all scintillation, and by 10 the highest degree, which is seen very rarely, and only when the star is near the horizon, when it sometimes scintillates very strongly, changes colour, and sometimes even disappears and appears again.   With a little practice it soon becomes easy to distinguish degrees of scintillation even between 0 and 1, 1 and 2, &c.   The next step is to ascertain the scintillation with still more exactitude, and to designate it as 0·4, 1·6, &c., though it is scarcely possible to subdivide these degrees further than 1, 4, or 5.

It may appear that this division is an arbitrary one, and that it is difficult to appreciate by figures a phænomenon like the scintillation of the stars : but this mode of proceeding is indicated by several learned men ; among others by the celebrated Horace Benedict de Saussure, who employed a similar means of estimating the famous dry fog of 1783.

De Saussure gives the amount of intensity sometimes as 3,

sometimes as 4, and sometimes as 8, &c., and gives the follow-
ing reasons for so doing :—" This scale that I have employed
is an imaginary division for the estimation of a phænomenon for
which we can have no real measure. I suppose, then, that the
highest degree is 10, the lowest 1; and I shall endeavour to
determine the intermediate degrees either by the intensity of the
sensation produced, or by other circumstances connected with
the phænomenon. This appears to me to present ideas with
more precision than by simply qualifying the different degrees
by the vague terms of strong, middling, and feeble. Thus I
would estimate at 8 degrees the fog of July 3, 1783." (De Saus-
sure, Travels in the Alps: Third Journey, chap. 2.)

I have explained the scale I employ; other observers can form
their own, according to their ideas of what is best. The num-
bers can be changed at will, so long as the degrees remain prac-
tically the same. Thus, my brother Mark, who began his ob-
servations about a year ago, has adopted a scale much lower than
mine; but we always agree as to the amount of scintillation on
any given evening, and also as to whether one star scintillates
more than another. These are the most important points to
decide on; for, as all the observations cannot be taken by the
same person, it would often be difficult to ascertain if the scin-
tillation is of the same intensity at Cairo, New Archangel, on
the Peak of Teneriffe, or at Morges.

Within certain limits this difficulty can be obviated, as I will
endeavour to show later; but in any case, by following the
method of instruction I have indicated, it will be possible to
know how much the scintillation varies from one night to an-
other, and whether the amount appears affected or is influenced
by any meteorological perturbations.

It is of course needless to add, that the height of the star ob-
served must be known and noted. It is not necessary, however,
to take a direct observation of the height of the star in question;
it is much easier to calculate this from the time of the night;
and, to abridge these calculations, a Table can be drawn up indi-
cating what is the exact height of the stars to be observed in the
different sidereal hours, according to the latitude of the obser-
ver's position. The Table I have gives the degrees of height for
Morges, calculated at intervals of half an hour. And this Table
is sufficient; for in the space of half an hour one can interpolate
with all the exactitude necessary, as in this case it is only need-
ful to know approximatively, within a degree or half a degree, the
height of the stars.

2nd. *Mode of comparing observations.*

It has been proved that, all other circumstances being equal,
the scintillation of the stars decreases in proportion as they ap-

proach the zenith; and reasoning from this fact, it may seem impossible to compare observations unless they have all been taken from stars at the same height. On comparing a vast number of observations taken under most favourable conditions, and when there had been no apparent atmospheric perturbations either on the preceding or following days, I have placed it beyond doubt that the scintillation does *really* decrease when the stars approach the zenith, and that, for any given height, the scintillation is sensibly proportional to the product obtained by multiplying the depth of the stratum of air which the rays of light traverse, by the astronomical refraction for the height under consideration.

Let us designate this product by P. Representing by 1 the height of the atmosphere, and estimating the refractions in seconds, it will be found that for the different heights the results P are—

Height of the star.	Value P.
20°	444
25	286
30	198
35	143·1
40	106·9
45	81·8
50	63·6
55	49·7
60	38·7
65	30·3
70	22·5
75	16·1
80	10·4
85	5·1

These figures represent tolerably well the normal state of the scintillation at Morges, when the height of the star above the horizon varies from 20° to 75°. Below 20° the calculated value no longer corresponds with the observations, but the neighbourhood of the horizon sufficiently explains this deviation; and as to the stars situated at a height which exceeds 75°, their scintillation is in general so feeble that the slightest error in the observation will cause a notable modification in the correspondence of the figures. Thus, if one has observed at a height of 60° a scintillation of 1·6, and one wishes to know what would have been, under the same circumstances, the scintillation of the same star if it had been only 45° above the horizon, one would obtain

$$\text{Scintillation at } 45° = \frac{1·6 \times 81·8}{38·7} = 3·4.$$

3rd. *Errors to be avoided.*

It often happens that from one night to another the scintillation varies very considerably; but it augments or diminishes proportionably for all the stars, except perhaps for those which, being nearest the horizon, have always a strong scintillation, or except when accidental causes modify it momentarily. Among these accidental causes we may cite, first, the twilight, which almost always very much augments the scintillation; and secondly, the neighbourhood of clouds. I think M. Kaëmtz was the first to notice that the scintillation augments when there are clouds driven by the wind. This is the fact, as I have observed it in thousands of cases, and I do not remember noticing a single exception.

Hence I do not say that we ought to reject observations taken when the stars are near clouds; only we must bear in mind this circumstance, as it greatly modifies the results obtained. A bright moonlight is also very unfavourable; for observations taken when the moon is full, are much less exact than those taken when she is absent.

4th. *Comparison of the observations made by different persons.*

This is the delicate point to consider; for what precedes suffices for the study of the phænomenon when all the observations are taken by a single person; but when there are several, how are we to know if the scintillation designated 2·5 is equal to that of another person also designated by 2·5? I believe it is impossible to obtain this unison; and unless every observer could be taught by one single person experienced in the matter, I am certain it never will be obtained. However, here is a method of recognizing if the scintillation is, in absolute value, stronger in one station than in another :—

At Morges, during the nights of maximum scintillation, the stars at the zenith scintillate very decidedly. In the nights of middling scintillation the stars in the same position scintillate feebly, though always enough to be appreciable; but in the nights when the scintillation is at its minimum, the stars nearest the zenith have no longer any scintillation at all; and the nearer the scintillation approaches to its minimum, the more extended is the spherical segment (of which the zenith is always the centre), which comprehends all those stars of which the scintillation is inappreciable. I have sometimes seen, when the scintillation was very feeble, that the stars lost all scintillation as soon as they were at 42° above the horizon; but I have never seen it cease entirely for stars less elevated, though, from what Arago says, it appears that that happens sometimes.

He names, among others, the observations of M. de Hum-

boldt, who says that on the borders of the Orinoco no scintillation can be observed, not even at 4° or 5° above the horizon.

Le Gentil asserts that at Pondicherry, during the months of January and February, the stars do not scintillate at all. Beauchamp wrote to Lalande, that at Bagdad the stars ceased to scintillate as soon as they arrived at 45° above the horizon.

Garcin asserted, in 1743, that at Bender Abassi, on the borders of the Persian Gulf, in the spring, summer, and autumn the stars did not scintillate; it was only in winter that a slight scintillation was perceptible.

According to M. de Humboldt, at Cumana in general the scintillation is no longer sensible when the stars are more than 25° above the horizon, &c. Most assuredly I have never seen at Morges, during the past six years, so small an amount of scintillation; but it is by specifying the various heights at which stars cease to scintillate that my observations are susceptible of being compared with those of M. de Humboldt and Le Gentil. I therefore call the attention of observers specially to this point, as probably the best manner of comparing the calculations obtained in various parts of the globe.

The scintillation of stars of the first magnitude is perfectly appreciable by the naked eye; that of the secondary ones less so; and so on, until for the smallest and least brilliant stars it becomes quite inappreciable : but this limit varies from one night to another, according as the general scintillation is strong or feeble. This fact may also serve as a means of comparing the degrees of scintillation.

The magnitude of the stars that appear to scintillate must be noted; and as we have seen that the height of the stars above the horizon exercises a great influence on the intensity of the phænomenon, it will be necessary to name some of the stars, and to indicate, besides, their height above the horizon, or at least the time at which the observation was taken; but I attach less importance to this method of comparison than to the preceding one, because the purity of the atmosphere, the sight of the observer, and the various degrees in the brilliancy of the stars, all exert more or less influence on the results obtained.

5th. *Variable Stars.*

Of all the stars I have observed, α of Orion is the one of which the scintillation appears to me to be the most irregular; but it is well known that the brilliancy of this star is not always the same : and with reference to the variable stars, all that is known of them is the duration of their periods; and consequently their scintillation is also an interesting phænomenon to study. It would be, above all, interesting in the case of the star η of Argo, whose

singular variations of brilliancy have so much surprised astronomers for the last thirty or forty years. Unfortunately this star is only visible at 31° north latitude ; and to observe it at a height of 30° above the horizon, one must travel as far as the equator, so that this research must be left to the astronomers of the other hemisphere.

6th. *Scintillation of the Planets.*

It is generally believed that the planets do not scintillate at all, or scarcely at all. Nevertheless I have often observed a sensible scintillation of Venus and Mars, and in a few rare cases I have also observed a slight scintillation of Jupiter and Saturn. For those persons who undertake to explain the phænomenon of the scintillation, it would be important to know if really the scintillation of these planets ever becomes very decided.

I would therefore call the attention of observers who may find themselves under atmospherical conditions of a nature to render the general scintillation very strong, to this point, as they might perhaps be able to ascertain whether Jupiter and Saturn ever sensibly scintillate.

7th. *Accidental Observations.*

In order to complete the study of this phænomenon, exceptional circumstances must not be neglected :—Among others, observations taken during an aurora borealis, both of the stars which appear plunged in the light of the aurora, and of the others in other parts of the heavens. During six years I have never been able to make any observations of this nature at Morges.

The observation of the scintillation from the summit of a high mountain. De Saussure made several during the time he was on the " Col du Géant ;" but those which are given in the works of this celebrated man are too few in number, and not detailed sufficiently to be able to draw from them any certain conclusions.

Mr. Piazzi Smyth remembers that the scintillation appeared to him very feeble from the summit of the Peak of Teneriffe ; and during the time I was staying on the Great St. Bernard, I remarked that the general scintillation was always very feeble. Is this always the case ?

8th. *Accessory Observations.*

In order to render the observations on the scintillation really interesting and useful, it is important that they should be accompanied by meteorological observations as complete as possible. At least let the state of the barometer, hygrometer, and thermometer be noted exactly ; also the state of the heavens, and the force and direction of the wind.

It is probable that wherever any observations are taken, meteorological observations are also taken and published; so that it would not be adding materially to the work of observers of the scintillation to ask them to add those observations to their own.

There are many more details which I pass over, because they depend on the peculiarity of the sight of different observers, and on the circumstances under which they may be placed, and which of course vary in every individual case : I have contented myself with indicating the principal points, to which I call the attention of observers, and the importance of which I know by experience.

In concluding, let me be permitted to express a wish, addressed to all who may be disposed to observe the scintillation of the stars in countries whose climates are different to that of Morges ; and that is, that they would kindly communicate to me a brief summary of their work—for it is needless for me to say that any result that they may arrive at will be of the highest interest to me ; and reciprocally, if they desire it, I shall have great pleasure in giving any further details or directions to any persons who will interest themselves in this research.

Morges, December 1859.

XXX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 158.]

May 26, 1859.—Sir Benjamin C. Brodie, Bart., Pres., in the Chair.

THE following communication was read :—
"On the Laws of Operation, and the Systematization of Mathematics." By Alexander J. Ellis, Esq., B.A., F.C.P.S.

The object of the following investigation is to give a firmer basis to the calculus of operations, to assign the strict limits and connexion of the mathematical sciences, and to found them upon purely inductive considerations, without any metaphysical or *a priori* reasoning.

Starting with the indemonstrable but verifiable hypothesis, that objects exist external to the subject, we recognize equality as existing between objects with common and peculiar properties, in respect of their common properties. Operations, which, when performed on equal objects, produce equal objects as their result, are recognized as equal, in respect to the common properties considered in the equalities of the objects. When one operation is performed on an object, and another on the resultant object, the single operation by which the first object is transformable into the last is regarded as the *product* of the other two, the order of succession being important. When the resultant object is the same as the original operand, the product of the operations is termed *unity*. When two operations performed

on the same object produce different resultant objects, the operation of transforming one of these resultant objects into the other, is regarded as the *quotient* of the two former operations. Two operations are termed *reciprocal* when their product is unity. Hence the quotient of two operations is the product of the one and of the reciprocal of the other. When two objects are combined in any manner so as to produce a third, and the two first are formable from any fourth by two known operations, the single operation by which the third object can be also formed from the fourth, is termed the same combination of the two first operations. From this we gain the conception of *null* or *zero*, as the operation of annihilating any object in respect to any place. The product of a combination of two operations and a third operation, is the same combination of the products of each of the combined operations severally and the third operation, in the particular order thus specified, provided all the operations and products are performable on the same operand.

The above general conceptions and laws of combined operations hold for any operations whatsoever with their appropriate operand objects; but the nature of the operations and operands requires especial study. In *mathematics*, objects are only considered with respect to their three most general properties: first, as contemplatable in discontinuous succession, whence number and *Arithmetic*; secondly, as contemplatable in continuous succession, whence extension and *Geometry*; and thirdly, as contemplatable in a continuous succession bearing a relation to another continuous succession, whence motion in time and *Mechanics*. The problem of mathematics is, first, to discover the laws of these successions as respects results (that is, statically), by means of considerations drawn from contemplating operations (that is, dynamical); secondly, to investigate the relations of these laws, giving rise to statical algebra; thirdly, to reduce all dynamical to statical laws, as in dynamical algebra; and fourthly, to make the expression of all the results dependent on the most simple, viz. those of common arithmetic. The purpose of the problem is to prepare the mind for the further investigation of nature, and to increase practical power immediately.

In *Arithmetic* we conceive objects spread out in a *scale*, and by aggregating those contained between any one and the beginning of the scale, form statical groups, whose distinctive character is derived from the scale. The operation by which any group is formed from the first object is termed an *integer*, the especial laws of which are next investigated. All objects being interchangeable in respect to discontinuous succession, an aggregate is not changed by altering the disposition of its parts. This leads to the first two *laws of commutation and association in addition*. The possibility of arranging objects at once in two horizontal directions, and a third vertical direction, leads to the *laws of commutation and association in multiplication*. Combining these with the two former, we have the *law of commutative distribution*. From the laws of association in multiplication is immediately deduced the *law of repetition* or indices.

Having obtained these laws, we proceed to study their relations in

the *algebra of integers,* first, statically, in order to reduce all results to the form of a numerical integer ; secondly, dynamically, considering the effect of a variation in the integer employed. This leads to the conception of a *formation* (Lagrange's "analytical function"), as a combination of a fixed and independently variable integer. Such a combination is, therefore, also itself dependently variable. The *inversion of formations,* whereby the independent variable is expressed as a formation of the dependent variable, immediately engages our attention. The inversion of a sum leads to a difference, with the limitation that the minuend should be greater than the subtrahend. The inversion of a product leads to a quotient, with the limitation that the dividend should be a multiple of the divisor. The inversions of a power lead to the root and logarithm, with increasing limitations. The study of discontinuous objects then allows the application of these inversions to the solution of problems in common life.

The operation by which any group in the arithmetical scale already described is formable from any other group in the same scale, leads to the conception of a *fraction,* necessarily expressible, according to the general laws of operation, as the quotient of two integers. The operands of such operations must admit of being separated into certain numbers of equal parts, or rather, in order that they may admit of *any* fractional operation, into *any* number of equal parts. Thus discontinuous approaches continuous succession. The *laws of fractions* are the same as the laws of integers, provided the indices used are all integers. The object of the statical *algebra of fractions* is to reduce all combinations of numerical fractions to numerical fractions. The inversion of formations is less limited than before. There is the same limitation respecting differences, but none respecting quotients. The attempt to convert all fractions into radical fractions (whose denominators are some powers of the radix of the system of numeration), leads to the conception of convergent infinite series, and hence allows an approximation to the inversion of a power with a constant index.

In *Geometry,* the notion of continuous succession or extension is derived from the motion of the hand, which recognizes separable but not separated parts. This motion gives the conception of surfaces, which by their intersections two and two, or three and three, give lines and points. Recognizing a line as the simplest form of extension, we distinguish the straight lines, which coincide when rotated about two common points, from the curves, which do not. These straight lines are shown to be fit operands for the integer and fraction operations. By moving one coinciding line over another so as to continue to coincide (by sliding), or to have one point only in common (by rotating), or no points in common (by translation), we obtain the conceptions of angles and parallels, which suffice to show that the exterior angle of a triangle is equal to the two interior and opposite, and that two straight lines meet or not according as the exterior angle they make with a third is not or is equal to, the interior angle. Angles are then considered statically as amounts of

rotation not exceeding a semi-revolution. Proceeding to examine the relations of triangles and parallelograms, we discover the operation of taking a fraction of a straight line, and therefore of a triangle and of any rectilineal figure. We see that this operation is, in fact, the same as that of altering a third line into a fourth, so that the multiples of the third and fourth, when arranged in order of magnitude, should lie in the same order as those of the first and second when similarly arranged. The relation of two magnitudes, with respect to this order, we term their *ratio*, and the equality of ratios *proportion*. The inversion and alternation of the four terms of a proportion are now investigated. The operation of changing any magnitude into one which bears a given ratio to it, is called a *tensor*. The *laws of tensors*, being investigated, are shown to be the same as those of fractions. They, however, furnish the complete conception of infinite and infinitesimal tensors, by letting one or other of the magnitudes by which the ratio is given become infinite or infinitesimal. Thence is developed the law, that tensors differing infinitesimally are equal for all assignables. Consequently tensors may be represented by convergent series of fractions. The *algebra of tensors* allows of the inversion of a sum with the same limitation as in the case of fractions, the complete inversion of a product of tensors, and the practical inversion of a power with a constant integral index. This algebra applied to geometry allows of the investigation of all statical relations, that is, of all the *geometry of the ancients*, in which magnitudes alone were considered, without direction. In respect to areas, the consideration of the parallelogram swept out by one straight line translated so as to keep one point on another straight line, leads to an independent *algebra of areas*, in which the generating lines are considered immediately. The laws of the relations of lines thus discovered, are shown to be identical with the laws of the relations of tensors. Consequently, with certain limitations, the whole of the algebra of tensors may be interpreted as results in the algebra of areas. This leads to a perfect conception of the principle of *homonomy*, or dissimilar operations having the same laws, and consequently the same algebra.

In *dynamical* or modern *geometry*, all lines are considered as in construction, having initial and final points. If the initial points of any two straight lines are joined to a third, not on either, and the two parallelograms be completed, the lines drawn from the point parallel to the given lines are dynamically equal to them ; if these last lie on each other, the first two lines have the *same direction* ; if the last have only one point in common and lie in the same straight line, the first have *opposite directions* ; and if the last do not lie in the same straight line, the first have *different directions,* and the angle between the last is the angle between the first lines. Similar definitions can be given of direction in the case of angles and circular arcs. If from the final point of any line we draw a line equal to a second, and join the initial point of the first with the final point of the line thus drawn, we are said to *append* the second to the first, and the joining line is called the *appense* of the other two. The

laws of appension are shown to be the same as those of addition, and are hence expressible by the same signs of combination, the difference in the objects combined preventing any ambiguity. We thus get the conception of a point as an annihilated line.

The tensor operation, considered dynamically, leads to the operation of changing a line dynamically so that it should bear the same relation to the result as two given lines bear to each other in magnitude and direction. This assumes three principal forms according to the difference of direction. If there is no difference of direction, the operation is purely a tensor. If the directions differ by a semi-revolution, the rotation of one line into the position of the other may take place on any plane. The operation is then termed a *negative scalar*; the tensor, which includes the operation of turning through any number of revolutions, is distinguished as a *positive scalar*. If the rotation be through any angle, but always on the same plane, the operation is here termed a *clinant*. If the rotation may take place on any variable plane, the operation is a *quaternion*.

The *laws of scalars* are immediately proved to be the same as those of tensors, but in addition they introduce the idea of negativity. This enables us in the *algebra of scalars*, to invert a sum generally, and thus allows of a perfect inversion of the first two formations. But a power with a fixed integral exponent can only be inverted on certain conditions. This partial inversion, however, leads to a solution of quadratic equations, and to a proof that formations consisting of a sum of integral powers, cannot be reduced to null by more scalar values of the variable than are marked by its highest exponent. Hence if such a formation is always equal to null, all the coefficients of the variable must be null. We thus obtain the method of indeterminate coefficients, by which we are enabled to discover a series which obeys the laws of repetition with respect to its variable, and becomes equal to a power when its variable is an integer. This enables us to define a power with any index, as this series, and hence to attempt the inversion of powers with variable indices, which we succeed in accomplishing under certain conditions. This investigation introduces the logarithm of a tensor, powers with fractional and negative exponents, and the binomial theorem for these powers. It also induces us to consider the *laws of formators*, or the operations by which a formation of any variable is constructed. They are shown to be commutative and associative in addition, associative in multiplication, directly distributive and repetitive, but not generally commutative in multiplication, nor even inversely distributive. When formators are commutative in multiplication and distribution, they are entirely homonomous with scalars, which may even be considered as a species of formators. The results of the former investigation, therefore, show that logarithms, fractional and negative powers, and the binomial theorem hold for these commutative formators.

The necessity of tabulating logarithms and of approximating to the solutions of equations, leads to the consideration of a method of deriving consecutive values of formations for known differences of the variable, and of interpolating values of the same formation for

intermediate values of the variable; that is, the *algebra of differences.* Considering the two operations of altering a formation by increasing the variable, and taking the difference between two different values of the formation (of which operations the first is necessarily unity added to the second), we regard them as formators, and immediately apply the results of that algebra, which furnishes all the necessary formulæ. For approximating to the roots of equations, we require to consider the case where the variable changes infinitesimally, thus founding the *algebra of differentials,* which is, in fact, a mere simplification of that of differences, owing to all the results being ultimately calculated for assignables only. Finally, to find the alteration in a formation of commutative formators, when the variable formator is increased by any other formator, we found the *algebra of derivatives.*

In applying the results of *scalar* algebra to *geometry,* we start with the fundamental propositions that the appense of the sides of an enclosed figure taken in order is a point, and that when the magnitude and direction of the diagonal of a parallelogram or parallelopipedon, and lines parallel the sides which have the same initial point as the diagonal, are given, the whole figures are completely determined. In order to introduce scalars, a unit-sphere is imagined, with its radii parallel to the lines in any figure, and in known directions. Any line can then be represented as the result of performing a scalar operation on the corresponding radius.

The first object is to reduce the consideration of angles to that of straight lines, by the introduction of cosines and sines, which are strictly defined as the scalars represented by the relation of the abscissa to the abscissal radius, and the ordinate to the ordinate radius respectively. These definitions immediately lead to the relations between the cosines and sines of the sums of two angles, and those of the angles themselves, whatever be their magnitude or direction, and thus found *goniometry.*

Defining a *projection* of any figure on any plane to be that formed by joining the points on that plane corresponding according to any law with those of the figure, we have the fundamental relation that, if the first, and therefore the second figure is enclosed, the appense of the sides of the second in the order indicated by the sides of the first, is a point. The orthogonal projection of any figure, by means of planes drawn perpendicular to any line, being all in one line, each projection can be represented as the result of a scalar operation performed on the same unit radius, and hence this projection leads to one invariable relation between scalars. By choosing three lines at right angles to each other on which to project, we obtain three scalar relations from every solid figure. If the figure is plane, then by projecting on a line and on a perpendicular to that line, we get two scalar relations.

Applying these results to *transversals,* where a line parallel to one unit radius cuts several other unit radii, produced either way if necessary, we obtain, by considering *two* intersected radii, the results of

trigonometry, and by considering *three* or *four* intersected radii, those of *anharmonic ratios*.

As any line drawn from the centre of the unit-sphere may be considered as the appense of three lines drawn along or parallel to three given unit radii, it may be expressed as the sum of the results of three scalar operations performed on these radii respectively. By properly varying these three scalars, the final point of the line may be made to coincide with any point in space. But if there be a given relation between the scalars, then the number of points will be limited, and the whole number of the points constitutes the locus of the original concrete equation referred to the accessory abstract equation. The consideration of this entirely new view of *coordinate geometry* is reserved for a second memoir.

Proceeding next to the *laws of clinants*, we readily demonstrate that they are the same as the laws of scalars; they introduce a new conception, however, that of rotating through an angle not necessarily the same as a semi-revolution, that is, of a plane versor. By the concrete equation of coordinate geometry, it is immediately shown that all clinants can be expressed as the sum of a scalar, and of the product of a scalar by a fixed, but arbitrarily chosen versor. The simplest versor to select is the quadrantal versor, which, under the name of quadrantation, is now studied. The two addends of a clinant, considered as a sum, are called its scalar and vector; its two factors, considered as a product, are its tensor and versor. The laws of these parts are then studied.

The statical *algebra of clinants* has for its object the reduction of all combinations of clinants given in the standard form of the sum of a scalar and vector, to a clinant of the same form. The application of this to the series obtained for a general scalar power, leads to two series, called cosines and sines of the variables, as distinguished from the goniometrical cosines and sines of an angle, with which they are ultimately shown to have a close connexion, which can be rendered most evident by assuming as the unit-angle that subtended by a circular arc of the length of its radius. Studying these series quite independently of these relations to angles, we discover that they bear to each other the same relations as the goniometrical cosines and sines, and that if the least tensor value of the variable for which the cosine series becomes null, is known, all its other values can be found by multiplying this by four times any scalar integer. This last product must be added to the least tensor value of the variable for which both the cosine or the sine series become equal to given scalars, in order to find all the solutions of such equations. Supposing the values of such series tabulated by the method of differences for all scalar values of the variable, so that such least tensor values can always be found, we are now able to assign the meaning of any power whose base and index are both clinants, and the logarithm of any clinant. This enables us to invert completely all the simple formations, sum, product, power with variable base and constant index, or constant base and variable index; and hence to solve all equations

of four dimensions with clinant coefficients, and to show that every formation consisting of a sum of integral powers with clinant coefficients, can be expressed as a product of as many simple formations as is determined by the highest index of the variable. The cosine and sine series can also be generally inverted. The versor of any clinant having a known angle (which is always equal to the cosine of its angle added to the product of the sine of its angle into a quadrantal versor), can now be shown to equal the cosine series added to the sine series multiplied by a quadrantal versor, when the variable of the series is the scalar ratio of the angle of the clinant to the angle subtended by a circular arc equal to its radius. From this the ratio of the circumference to the diameter of a circle is shown to be twice the least tensor value of the variable, for which the cosine series is equal to null; and as that value can be readily assigned in a convergent series, the former ratio is determined. The same investigation shows the relation already mentioned between the goniometrical cosines and sines, and the cosine and sine series.

Clinant algebraical geometry allows us to interpret all results of clinant algebra when referred to lines on one plane. It thus furnishes a complete explanation of the "imaginary" points and lines in the theory of *anharmonic ratios,* when viewed in relation to the unit radii, as already explained. In the case of *coordinate geometry* of two, and even three dimensions, the possibility of interpreting the results of a clinant operation performed on a given unit radius in a given plane, allows us to understand the whole theory of "imaginary" intersections. The theory of *scalar and clinant algebraical coordinate geometry* will form the subject of a future memoir.

Proceeding to *quaternions,* we find their laws to be the same as those of clinants while the plane remains unaltered; but if the plane is alterable, they cease to be commutative in multiplication, that relation being replaced by one between certain related quaternions called their conjugates. This makes the *algebra of quaternions* (which is not here systematized, as being too recent) entirely different from that of scalars.

In *mechanics* the motion of any point is not considered absolutely as in dynamical geometry, but relatively to some external, constant, independent motion, as the apparent motion of the fixed stars ; this gives the conception of time. But the necessity of considering the motion not merely of a point, but of a body, gives rise to the comparison of the motions of various bodies, and to a conception of their equality, when the products of their velocities, multiplied by a constant which is always the same for the same body, but different for different bodies, are equal. This constant is the mass, which in bodies of the same kind varies as the volume.

By considering the case of the mutual destruction of motion, we eliminate time and simplify the problem, thus founding *statics* ; and by conceiving the motion of any body to be destroyed by the application of variable motions equal and opposite to those actually existent, we reduce *dynamics* to statics.

Nov. 17, 1859.—Sir Benjamin C. Brodie, Bart., Pres., in the Chair.

The following communication was read :—

"Notes of Researches on the Poly-Ammonias."—No. VI. New Derivatives of Phenylamine and Ethylamine. By A. W. Hofmann, LL.D., F.R.S. &c.

Some time ago* I communicated to the Royal Society some results obtained in studying the action of dibromide of ethylene upon phenylamine. The principal product of this reaction was found to be a well-defined crystalline compound with basic characters. By the analysis of the base itself, and of several of its combinations, it had been proved that the formula

$$C_{16} H_9 N = \left. \begin{array}{c} (C_4 H_4)'' \\ C_{12} H_5 \end{array} \right\} N$$
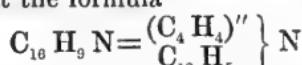
is the simplest atomic expression for the new substance ; but the action of iodide of methyle and of ethyle upon this body having given rise to compounds

$$C_{31} H_{21} N_2 I = \left. \begin{array}{c} C_{16} H_9 N \\ C_{16} H_9 N \end{array} \right\} C_2 H_3 I$$

and

$$C_{36} H_{23} N_2 I = \left. \begin{array}{c} C_{16} H_9 N \\ C_{16} H_9 N \end{array} \right\} C_4 H_5 I,$$
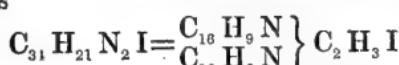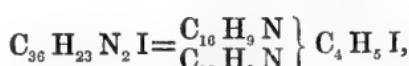
I was induced to assume the formula

$$C_{32} H_{18} N_2 = \left. \begin{array}{c} (C_4 H_4)'' \\ (C_{12} H_5)_2 \end{array} \right\} N_2$$
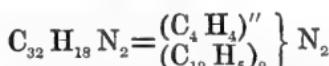
as representing the true constitution of the basic body, which thus appears as a diammonia, in which 2 equivs. of hydrogen are replaced by 2 equivs. of phenyle, and 4 equivs. of hydrogen by 2 molecules of diatomic ethylene.

This view involves the existence of a basic compound,

$$C_{28} H_{16} N_2 = \left. \begin{array}{c} (C_4 H_4)'' \\ (C_{12} H_5)_2 \\ H_2 \end{array} \right\} N_2,$$
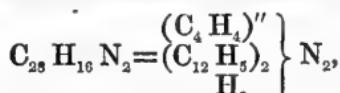
i.e. of a diphenyl-diamine in which only one molecule of diatomic ethylene has been substituted for hydrogen.

Experiment has not failed to realize the body pointed out by theory. A mixture of dibromide of ethylene with a large excess of phenylamine (1 vol. of dibromide of ethylene and 4 vols. of phenylamine) rapidly solidifies to a crystalline mass. Treatment with water removes from this mixture a very considerable proportion of hydrochlorate of phenylamine, leaving a brown resinous substance, which gradually but imperfectly solidifies. This substance forms a hydrochlorate which is difficultly soluble in concentrated hydrochloric acid, and which may be readily purified by repeated crystallizations from boiling alcohol. The pure hydrochlorate dissolved in water, and mixed with potassa or ammonia, furnishes the free base, which generally separates as an oil, rapidly solidifying into a cry-

* Phil. Mag. vol. xvii. p. 66.

stalline substance. This may be further purified by repeated crystallizations from diluted alcohol.

Analysis, in fact, assigns to this body the formula

$$C_{28} H_{16} N_2 = \left. \begin{array}{c} (C_4 H_4)'' \\ (C_{12} H_5)_2 \\ H_2 \end{array} \right\} N_2,$$
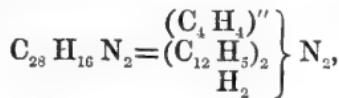
which was confirmed by the analysis of the dichloride—

$$\left[\begin{array}{c} (C_4 H_4)'' \\ (C_{12} H_5)_2 \\ H_4 \end{array} \right\} N_2 \right]'' Cl_2,$$

and of the platinum-salt—

$$\left[\begin{array}{c} (C_4 H_4)'' \\ (C_{12} H_5)_2 \\ H_4 \end{array} \right\} N_2 \right]'' Cl_2, 2 Ft Cl_2.$$
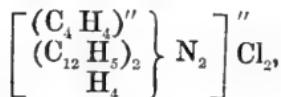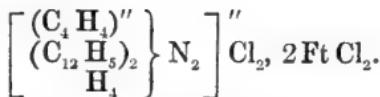
The formation of the new body is obvious:

$$4 \left[\begin{array}{c} C_{12} H_5 \\ H_2 \end{array} \right\} N \right] + (C_4 H_4)'' Br_2 = \left. \begin{array}{c} (C_4 H_4)'' \\ (C_{12} H_5)_2 \\ H_2 \end{array} \right\} N_2 + 2 \left(\left[\begin{array}{c} C_{12} H_5 \\ H_3 \end{array} \right\} N \right] Br \right).$$

Phenylamine. Dibromide of Ethylene- Bromide of
E hylene. diphenyl- Phenyl-ammonium.
diamine.
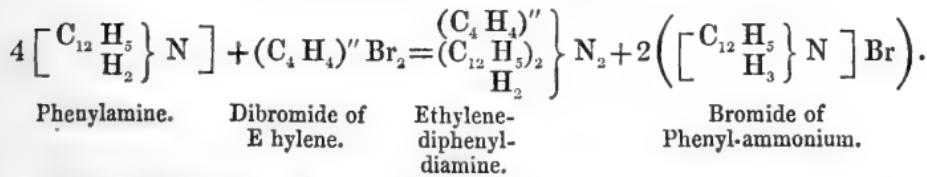
This substance differs in its physical characters essentially from the base containing 2 molecules of ethylene. The former is very soluble in alcohol and ether, the latter being very difficultly soluble; its fusing-point is 59°, the fusing-point of the latter being 157°.

In order finally to establish the relation between the body which forms the subject of this note and the base previously described, it remained to prove experimentally that the former, when submitted to the action of dibromide of ethylene, may be readily converted into the latter. Nothing is easier than to accomplish this transformation, which, in the presence of alcohol, is rapidly effected at the temperature of boiling water.

Treatment of the product of digestion with water removes the dichloride of ethylene-diphenyl-diammonium, the diethylene-diphenylamine remaining dissolved in the excess of dibromide of ethylene, from which it may be readily extracted by hydrochloric acid.

Preparation of the substance in a state of purity, and comparison of its properties with those of the body previously obtained, established beyond a doubt the transformation, which resolves itself into a simple process of substitution—

$$\left[\begin{array}{c} (C_4 H_4)'' \\ (C_{12} H_5)_2 \\ H_2 \end{array} \right\} N_2 \right] + (C_4 H_4)'' Br_2 = \left. \begin{array}{c} (C_4 H_4)_2'' \\ (C_{12} H_5)_2 \end{array} \right\} + N \left[\begin{array}{c} (C_4 H_4)'' \\ (C_{12} H_5)_2 \\ H_4 \end{array} \right\} N_2 \right]'' Br_2.$$
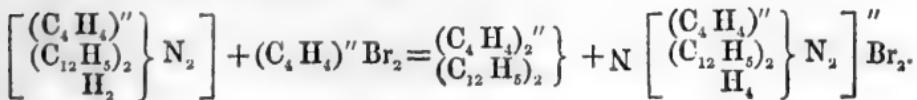
Ethylene-diphenyl-diamine being a secondary diamine, it was not without interest to replace the two remaining hydrogen-equivalents by two monatomic molecules. On digesting the base with iodide of ethyle some hours at a temperature of 100°, a beautiful iodide was obtained, crystallizing in well-defined prisms, difficultly soluble in water, but more soluble in alcohol.

This substance contains

$$C_{36} H_{26} N_2 I_2 = \left[\begin{matrix} (C_4 H_4)'' \\ (C_4 H_5)_2 \\ (C_{12} H_5)_2 \\ H_2 \end{matrix} \right\} N_2 \right]'' I_2.$$
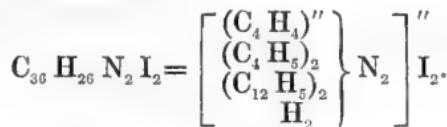
Treatment with potassa separates from this iodide the base as a crystalline body fusing at 70°, and resembling in many respects the previous base. It contains

$$C_{36} H_{24} N_2 = \begin{matrix} (C_4 H_4)'' \\ (C_4 H_5)_2 \\ (C_{12} H_5)_2 \end{matrix} \right\} N_2,$$
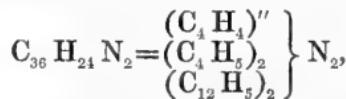
and forms a beautiful platinum-salt crystallizing in needles of the formula

$$[C_{36} H_{26} N_2]'' Cl_2, 2PtCl_2.$$
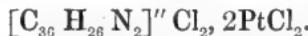
The deportment of phenylamine under the influence of dibromide of ethylene gives a fair illustration of the nature of the substances which are generated, under the influence of diatomic molecules, from primary aromatic monamines.

To complete the study of this subject, I have examined, moreover, the action of dibromide of ethylene upon ethylamine, as a representative of the monamines containing an ordinary alcohol-radical.

Dibromide of ethylene acts upon ethylamine even in the cold, the products of the reaction varying according to the relative proportions of the two bodies, and according to the temperature. Among other products invariably occur the two bromides corresponding to the two salts of the phenyl-compounds mentioned in the previous paragraphs.

These substances are the

Dibromide of ethylene-diethyl-diammonium, $\left. \right\} C_{12} H_{18} N_2 Br_2 = \left[\begin{matrix} (C_4 H_4)'' \\ (C_4 H_5)_2 \\ H_4 \end{matrix} \right\} N_2 \right]'' Br_2,$ and

Dibromide of diethylene-diethyl-diammonium, $\left. \right\} C_{16} H_{22} N_2 Br_2 = \left[\begin{matrix} (C_4 H_4)_2'' \\ (C_4 H_5)_2 \\ H_2 \end{matrix} \right\} N_2 \right]'' Br_2.$
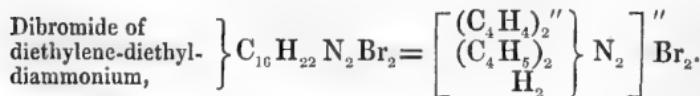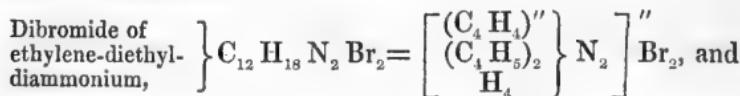
I have fixed the composition of the former compound by the analysis of the dibromide of the dichloride and of the base itself, all of which are remarkably well-defined crystalline bodies, and that of the latter by the examination of a well-defined platinum-salt.

The first base, separated by the action of anhydrous baryta from the dry bromide, distils as an oily liquid of a powerfully ammoniacal odour, which solidifies into a brittle crystalline mass not unlike fused stearic acid. The composition of the body is remarkable. It contains

$$C_{12} H_{18} N_2 O_2 = \left[\begin{array}{c} (C_4 H_4)'' \\ (C_4 H_5)_2 \\ H_4 \end{array} \right\} N_2 \right]'' O_2,$$
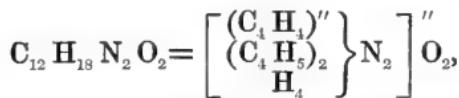
and thus constitutes the dioxide of the diatomic metal, ethylene-diethyl-diammonium.

The second base is liquid, and boils at 185°. It is easily obtained from the dibromide, which, being extremely soluble, may be readily separated from the bromide of the first body. I have experimentally established that this body may be readily procured by the action of dibromide of ethylene upon the dioxide previously mentioned.

The dioxide,

$$C_{12} H_{18} N_2 O_2,$$

presents considerable interest in a theoretical point of view. I have determined the vapour-density of this compound by Gay-Lussac's process. Experiment gave the number 2·26. Assuming that the molecule of the body under examination corresponds to 4 volumes of vapour, the theoretical density is 4·62.

The extraordinary discrepancy between theory and experiment may be removed in two ways: viz. either by halving the formula, or by assuming that the molecule of the dioxide of ethylene-diethyl-diammonium corresponds to 8 volumes of vapour, in either of which cases the theoretical density becomes 2·31, closely agreeing with the experimental number 2·26.

I shall discuss the vapour-densities of the diammonias somewhat more fully in a future communication; but I cannot refrain from pointing out even now, that, by dividing the formula by 2, we arrive at an expression containing 1 equiv. of oxygen (O=8), which, in the eyes of those who consider the number 16 as the true molecular value of oxygen, must appear perfectly inadmissible.

GEOLOGICAL SOCIETY.

[Continued from p. 162.]

January 18, 1860.—Sir C. Lyell, Vice-President, in the Chair.

The following communications were read:—

1. "Notice of some Sections of the Strata near Oxford." By John Phillips, M.A., LL.D., F.R.S., Pres. G.S. &c.

From the Yorkshire coast to that of Dorset, evidence of unconformity between the Oolitic and the Cretaceous strata is readily observed, the latter resting on several different members of the former along this tract. This is especially seen in the neighbour-

hood of Oxford, where it is difficult to trace out correctly the limits of the Lower Cretaceous beds. The Oolitic rocks having been deposited whilst the relative position of the land and sea was being changed, many of the deposits are subject to local limitation; thus the Coralline, Oolitic, and the Calc-grit die out rapidly, and the Kimmeridge Clay comes to rest on the Oxford Clay. It is on the surface formed by these irregular beds, and that surface considerably denuded, owing to elevations before the Oolitic period was ended, that the Lower Cretaceous beds have been laid down. From their close propinquity, the sand-beds of different ages, when without fossils, are scarcely to be defined as Oolitic or Cretaceous, and where one clay lies upon a similar clay, the occurrence of fossils only can secure their distinction.

The Farringdon sands, the sands of Shotover Hill, and those near Aylesbury, are still open to research,—their Lower Greensand characters not having been clearly established. At Culham, a few miles south of Oxford, a clay-pit is worked, which presents, at the top, 3 feet of gravel; next about 20 feet of Gault with its peculiar fossils; then 9 feet of greenish sand, with a few fossils; and lastly 23 feet of Kimmeridge Clay, with its peculiar Ammonites and other fossils. In winter the clay-pit, being wet, offers little evidence of any distinction between the upper and the lower parts of the clay; but in summer the Gault and its fossils are more easily recognized. The intervening sand contains *Thracia depressa, Cardium striatulum,* and an Ammonite resembling one found in the Kimmeridge Clay. Although this sand at first sight resembles the Lower Greensand, yet it is probably more closely related to the Kimmeridge Clay. Puzzling as this sand is in the pit, another enigma is offered by the railway-section at Culham, where the Kimmeridge Clay is overlaid by a sand perhaps equivalent to that of Shotover Hill, not that of the clay-pit; whilst the Gault, which lies on it unconformably, can be connected with that of the clay-pit. At Toot Baldon also, though Lower Greensand probably caps the hill, yet an Oolitic Ammonite was found on the eastward slope of the hill, in a ferruginous sand, lying conformably on the Kimmeridge Clay. From these and other instances the difficulty of mapping the country geologically may be shown to be very great,—the sands of any one bed differing in colour from green to red, according to the amount of oxidation produced by exposure and other causes; and if fossils are absent, the Portland Sand and the Lower Greensand, lying against each other, may never be defined. From the great and irregular denudation, too, of the rocks, and the unequal deposition of many of the beds, it will prove a difficult problem to trace the several sands and define their age,—a problem to be solved only by close perseverance and strict search for organic remains.

2. "On the Association of the Lower Members of the Old Red Sandstone and the Metamorphic Rocks on the Southern Margin of the Grampians." By Prof. R. Harkness, F.R.S., F.G.S.

The area to which this paper referred is the tract lying between

Stonehaven and Strathearn, including the south-eastern flanks of the Grampians for about two-thirds of their course. Metamorphic rocks, trap-rocks, the Lower and Middle members of the Old Red Series (the former being sandstone, and the latter conglomerate), are the constituent rock-masses of the district, and give it its peculiar physical features. The mode in which these rocks are associated is well exhibited in the section on the coast (at Stonehaven), and in the several sections in the interior where streams lay bare the rocks. Sections at Stonehaven, Glenburnie, Strathfinlass, North Esk, West Water of Lithnot, Cruick Water, South Esk and Prosen, Blairgowrie, Dunkeld, Strathearn, and Glenartney, were described in detail.

Against the nearly vertical, but somewhat north-westerly dipping, metamorphic schists (which sometimes include conformable limestones), come purple flagstone, but usually separated from them by trap-rocks, having the same strike. These flagstones pitch to the south-east, but retain a high angle away from the schists, and, in many places, are intercalated with beds of trap. The lower purple flagstones are unfossiliferous; but higher up tracks of Crustaceans (*Protichnites*) have been discovered by the Rev. H. Mitchell. The grey fossiliferous flagstones of Forfarshire succeed, still with a steep dip. Conglomerates succeed, in beds having a less inclination, gradually becoming more and more horizontal as they reach the low country.

The axis of the elevation of the Grampians thus appears to be along their southern margin, and to be marked by the trap-rocks separating the metamorphic schists and the purple flagstones of the Old Red series, and giving the latter their general south-easterly dip. As the metamorphic rocks of the Grampians have not yielded any fossils, their relation to the other old rocks of Scotland is difficult to determine.

3. "On the Old Red Sandstone of the South of Scotland." By Archibald Geikie, Esq., F.G.S., of the Geological Survey of Great Britain.

This paper was the result of a series of explorations carried on at intervals from Girvan to St. Abb's Head. The first part related to the geology of the border-district of Lanark and Ayr, near Lesmahagow. The Silurians and Lower Old Red sandstones of that district, as formerly pointed out by Sir Roderick Murchison, form one consecutive series. They are traversed by great numbers of felstone-dykes, and are disposed in longitudinal folds, ranging from N.E. to S.W., the Silurian strata forming the axis of each anticline. Both series are overlaid unconformably by Carboniferous strata belonging to the horizon of the Mountain Limestone group of Scotland. The features of this unconformity are well displayed all round Lesmahagow, where an enormous series of Lower Old Red sandstones, more than 10,000 feet thick, have their truncated edges overlapped by gently inclined beds of Carboniferous sandstone, shale, and limestone. The whole of the Lower Carboniferous group and the upper Old Red Sandstone, amounting in all to at least 6000 or

8000 feet, are here wanting. But as the junction of the Carboniferous Limestone with the Lower Old Red is traced towards the east, the thickness of strata between the two formations gradually increases, until at the Pentland Hills the whole of the Lower Carboniferous series and a considerable part of the Upper Old Red have come in ; and these strata, as at Lesmahagow, rest quite unconformably on the base of the Lower Old Red Sandstone and the higher beds of the Upper Silurian. Hence it becomes apparent that in the south of Scotland, as in Ireland, there is a great physical break between the Upper Old Red Sandstone and the lower part of that formation.

The author next pointed out the character of the Upper Old Red Sandstone in East Lothian and Berwickshire ; showing that it graduated by imperceptible stages into the Lower Carboniferous sandstones, and formed with these one great lithological series. The former wide extension of the Upper Old Red Sandstone throughout the south-east of Scotland was shown by the height at which it occurs among the Lammermuirs. These hills must unquestionably have been covered by it ; and hence the denudation of the south of Scotland will eventually be shown to be one of the greatest which this country has undergone. The author concluded by sketching the physical geography of South Scotland during the Upper Old Red Sandstone period, in so far as it was indicated by the facts presented in this paper. He showed that the rate of subsidence was probably much greater in the eastern than in the western districts, inasmuch as the whole of the vast series of Upper Old Red and Lower Carboniferous sandstones had accumulated in the Lothians and Berwickshire before the base of the Lesmahagow hills began to be washed by the waves of the encroaching sea.

ROYAL INSTITUTION OF GREAT BRITAIN.

January 20, 1860.—" On the Influence of Magnetic Force on the Electric Discharge." By John Tyndall, Esq., F.R.S.

The intention of the speaker was to bring before the meeting a series of experiments illustrative of the constitution of the electric discharge and of the action of magnetism upon it. The substance of the discourse was derived from the researches of various philosophers, its form being regulated to suit the requirements of the audience.

1. The influence of the transport of particles was first shown by an experiment suggested, it was believed, by Sir John Herschel, and performed by Professor Daniell. The carbon terminals of a battery of 40 cells of Grove were brought within one-eighth of an inch of each other, and the spark from a Leyden jar was sent across this space. This spark bridged with carbon particles the gap which had previously existed in the circuit, and the brilliant electric light due to the passage of the battery current was immediately displayed.

2. The magnified image of the coal-points of an electric lamp was projected upon a white screen, and the distance to which they could be drawn apart without interrupting the current was noted. A button of pure silver was then introduced in place of the positive carbon, a luminous discharge four or five times the length of the former being thus obtained. The silver was first observed to glow, and afterwards to pass into a state of violent ebullition. A narrow dark space was observed to surround one of the poles, corresponding probably with the dark space observed in the discharge of Ruhmkorff's coil through rarefied media*.

3. The action of a magnet upon the splendid stream of green light obtained in the foregoing experiment was exhibited. A small horse-shoe magnet of Logemann was caused to approach the light, which was bent hither and thither according as the poles of the magnet changed their position: the discharge in some cases formed a magnificent green bow, which on the further approach of the magnet was torn asunder, and the passage of the current thereby interrupted. It was Davy who first showed the action of a magnet upon the voltaic arc. The transport of matter by the current was further illustrated by a series of deposits on glass obtained by Mr. Gassiot from the continued discharge of an induction coil.

4. A discharge from Ruhmkorff's coil was sent through an attenuated medium; and the glow which surrounded the negative electrode was referred to. One of the most remarkable effects hitherto observed was that of a magnet upon this negative light. Plücker had shown that it arranges itself under the influence of the magnet exactly in the direction of the magnetic curves. Iron filings strewn in space, and withdrawn from the action of gravity, would arrange themselves around a magnet exactly in the manner of the negative light.

An electric lamp was placed upon its back ; a horseshoe magnet was placed horizontally over its lens, and on the magnet a plate of glass : a mirror inclined at an angle of 45° received the beam from the lamp, and projected it upon the screen. Iron filings were scattered on the glass, and the magnetic curves thus illuminated were magnified, and brought to clear definition upon the screen. The negative light above referred to arranges itself, according to Plücker, in a similar manner.

5. The rotation of an electric current round the pole of a magnet, discovered by Mr. Faraday in the Royal Institution, nearly forty years ago, was next shown ; and the rotation of a luminous current from an induction coil in an exhausted receiver, by the same magnet, was also exhibited, and both shown to obey the same laws. This beautiful experiment was devised by De la Rive.

6. Into a circuit of 20 cells a large coil of copper wire was introduced ; and when the current was interrupted, a bright spark, due to the passage of the extra current, was obtained. The brightness and

* Mr. Faraday noticed this dark stripe while the speaker was making his preparatory experiments.

loudness of the spark were augmented when a core of soft iron was placed within the coil. The disruption of the current took place between the poles of an electro-magnet; and when the latter was excited, an extraordinary augmentation of the loudness of the spark was noticed. This effect was first obtained by Page, and was for a time thought to denote a new property of the electric current.

But Rijke had shown, in a paper the interest of which is by no means lessened by the modesty with which it is written, that the effect observed by Page is due to the sudden extinction of the primary spark by the magnet; which suddenness concentrates the entire force of the extra current into a moment of time. Speaking figuratively, it was the concentration of what, under ordinary circumstances, is a mere push, into a sudden kick of projectile energy.

7. The contact-breaker of an induction coil was removed, and a current from five cells was sent through the primary wire. The terminals of the secondary wire being brought very close to each other, when the primary was broken by the hand, a minute spark passed between the terminals of the secondary. When the disruption of the primary was effected between the poles of an excited electro-magnet, the small spark was greatly augmented in brilliancy. The terminals were next drawn nearly an inch apart. When the primary was broken between the excited magnetic poles, the spark from the secondary jumped across this interval, whereas it was incompetent to cross one-fourth of the space when the magnet was not excited. This result was also obtained by Rijke, who rightly showed that in this case also the augmented energy of the secondary current was due to the augmented speed of extinction of the primary spark between the excited poles. This experiment illustrated in a most forcible manner the important influence which the mode of breaking contact may have upon the efficacy of an induction coil.

The splendid effects obtained from the discharge of Ruhmkorff's coil through exhausted tubes were next referred to. The presence of the coil had complicated the theoretic views of philosophers with regard to the origin of those effects; the intermittent action of the contact-breaker, the primary and secondary currents, and their mutual reactions, producing tertiary and other currents of a higher order, had been more or less invoked by theorists to account for the effects observed. Mr. Gassiot was the first to urge, with a water battery of 3500 cells, a voltaic spark across a space of air, *before* bringing the electrodes into contact; with the self-same battery he had obtained discharges through exhausted tubes, which exhibited all the phænomena hitherto observed with the induction coil. He thus swept away a host of unnecessary complications which had entered into the speculations of theorists upon this subject.

8. On the present occasion, through the kindness of Mr. Gassiot, the speaker was enabled to illustrate the subject by means of a battery of 400 of Grove's cells. The tension at the ends of the

battery was first shown by an ordinary gold-leaf electroscope; one end of the battery being insulated, a wire from the other end was connected with the electroscope; the leaves diverged; on now connecting the other end of the battery with the earth, the tension of the end connected with the electrometer rose, according to a well-known law, and the divergence was greatly augmented.

9. A large receiver (selected from Mr. Gassiot's fine collection), in which a vacuum had been obtained by filling it with carbonic acid gas, exhausting it, and permitting the residue to be absorbed by caustic potash, was placed equatorially between the poles of the large electro-magnet. The jar was about six inches wide, and the distance between its electrodes was ten inches. The negative electrode consisted of a copper dish, four inches in diameter; the positive one was a brass wire.

On the 16th of this month an accident occurred to this jar. Mr. Faraday, Mr. Gassiot, and the speaker had been observing the discharge of the nitric acid battery through it. Stratified discharges passed when the ends of the battery were connected with the electrodes of the receiver; and on one occasion the discharge exhibited an extraordinary effulgence; the positive wire emitted light of dazzling brightness, and finally gave evidence of fusion. On interrupting the circuit, the positive wire was found to be shortened about half an inch, its metal having been scattered by the discharge over the interior surface of the tube.

10. The receiver in this condition was placed before the audience in the position mentioned above. When the ends of the 400-cell battery were connected with the wires of the receiver, *no discharge passed*; but on touching momentarily with the finger any portion of the wire between the positive electrode of the receiver and the positive pole of the battery, a brilliant discharge instantly passed, and continued as long as the connexion with the battery was maintained. This experiment was several times repeated: the connexion with the ends of the battery was not sufficient to produce the discharge; but in all cases the touching of the positive wire caused the discharge to flash through the receiver.

Previous to the fusion of the wire above referred to, this discharge usually exhibited fine stratification: its general character now was that of a steady glow, through which, however, intermittent luminous gushes took place, each of which presented the stratified appearance.

11. On exciting the magnet between whose poles the receiver was placed, the steady glow curved up or down according to the polarity of the magnet, and resolved itself into a series of effulgent transverse bars of light. These appeared to travel from the positive wire along the surface of the jar. The deflected luminous current was finally extinguished by the action of the magnet.

12. When the circuit of the magnet was made and immediately interrupted, the appearance of the discharge was extremely singular. At first the strata rushed from the positive electrode along the upper

surface of the jar, then stopped, and appeared to return upon their former track, and pass successively with a deliberate motion into the positive electrode. They were perfectly detached from each other; and their successive engulfments at the positive electrode were so slow as to be capable of being counted aloud with the greatest ease. This deliberate retreat of the strata towards the positive pole was due, no doubt, to the gradual subsidence of the power of the magnet. Artificial means might probably be devised to render the recession of the discharge still slower. The rise of power in the magnet was also beautifully indicated by the deportment of the current.

After the current had been once quenched, as long as the magnet remained excited, no discharge passed; but on breaking the magnet circuit, the luminous glow reappeared. Not only, then, is there an action of the magnet upon the particles transported by an electric current, but the above experiment indicates that there is an action of the magnet upon the electrodes themselves, which actually prevents the escape of their particles. The influence of the magnet upon the electrode would thus appear to be *prior* to the passage of the current.

13. The discharge of the battery was finally sent through a tube whose platinum wires were terminated by two small balls of carbon: a glow was first produced; but on heating a portion of the tube containing a stick of caustic potash, the positive ball sent out a luminous protrusion, which subsequently detached itself from the ball,—the tube becoming instantly afterwards filled with the most brilliant strata. There can be no doubt that the superior effulgence of the bands obtained with this tube is due to the character of its electrodes: *the bands are the transported matter of these electrodes.* May not this be the case with other electrodes? There appears to be no uniform flow in nature; we cannot get either air or water through an orifice in a uniform stream; the friction against the orifice is overcome by starts, and the jet issues in pulsations. Let a lighted candle be quickly passed through the air; the flame will break itself into a beaded line in virtue of a similar intermittent action, and it may be made to sing, so regular are the pulses produced by its passage. Analogy might lead us to suppose that the electricity overcomes the resistance at the surface of its electrode in a similar manner, escaping from it in tremors,—the matter which it carries along with it being broken up into strata, as a liquid vein is broken into drops*.

* Mr. Gassiot has shown that *a single discharge* of the Leyden jar produces the stratification. May not every such discharge correspond to a single draw of a violin bow across a string?

XXXI. *Intelligence and Miscellaneous Articles.*

ON THE CORRELATION OF PHYSICAL, CHEMICAL, AND VITAL FORCE.

To the Editors of the Philosophical Magazine.

GENTLEMEN,

IN reference to the very interesting paper of Professor Le Conte's, published in the February Number of the Philosophical Magazine (on the Correlation of Physical, Chemical, and Vital Force), may I take the liberty of calling your attention to the fact that a view in many respects similar to his has been argued in a paper "On the Theory of Inflammation," contributed by me to the British and Foreign Medico-Chirurgical Review for July 1858?

Two or three sentences from that paper will sufficiently exhibit the points of agreement. After a reference to the partial decomposition in the egg and the seed, which is a condition of their development, and to other instances, these words follow :—" Such facts as these justify us in placing decomposition in organic tissues among the circumstances which give rise to the organizing process."—P. 215. At p. 222 is the following sentence :—"As a formative or vital process, dependent on a decomposing or chemical one, it [inflammation] corresponds to the clearest conception of nutrition that we can gather from the phænomena of life in all its forms."

It is a great satisfaction to me to see views to which I had been led, so much more ably and fully exhibited by an independent writer.

I am, Gentlemen,

Your obedient Servant,

JAMES HINTON.

London, February 1860.

ON THE CONDUCTIBILITY OF CERTAIN ALLOYS FOR HEAT AND ELECTRICITY. BY G. WIEDEMANN.

In an experimental investigation, Wiedemann and Franz[*] found that the thermal and electrical conductibility of metals was nearly identical. Their researches also showed that in brass (which contains 1 part of zinc to 2 of copper) the thermal conductibility differs but very little from that of the worse conducting metal, zinc, although the latter is present in smallest quantity. In other alloys, as those of tin and lead, an analogous relation prevails in reference to the electric conductibility. Messrs. Calvert and Johnson have lately investigated the thermal conductibility of several alloys, and have arrived at results which differ materially from those of Wiedemann and Franz, and which render doubtful the analogy which had been

[*] Phil. Mag. [4] vol. vii. p. 33 ; vol. x. p. 393.

established between the thermal and electrical conductibility. Wiede-
mann has accordingly determined the conductibility for heat and
electricity of several alloys. He used the same method as in the
previous researches, and the following Table contains the results at
which he has arrived. The standard adopted is silver, the conduc-
tibility of which, both for heat and electricity, is taken at 100.

Copper-zinc $\frac{8}{1}$ denotes an alloy containing 8 parts of copper to 1 of
zinc.

	Conductibility for	
	Heat.	Electricity.
Copper	73·6	79·3
Copper-Zinc $\frac{8}{1}$	27·3	25·5
Copper-Zinc $\frac{6·5}{1}$..........	29·9	30·9
Copper-Zinc $\frac{4·7}{1}$..........	31·1	29·2
Brass $\frac{2·1}{1}$..........	25·8	25·4
Zinc	28·1	27·3
Tin....................	15·2	17·0
Tin-Bismuth $\frac{3}{2}$	10·1	9·0
Tin-Bismuth $\frac{1}{1}$	5·6	4·4
Tin-Bismuth $\frac{1}{3}$	2·3	2·0
Rose's Metal	4·0	3·2

From these results Wiedemann concludes—

1. That the agreement, which had been previously found to exist,
between the thermal and electrical conductibility of metals obtains
also for alloys.

2. That the conductibilities of alloys of zinc and copper, for
heat as well as for electricity, differ but little, even with a consider-
able excess of copper, from the conductibility of the worse conducting
metal, zinc. The alloys of zinc and bismuth, on the contrary, have
nearly the mean conductibility calculated from their atomic com-
position.—*Poggendorff's Annalen*, Nov. 1859.

XXXII. *On the Effect of the Motion of a Body upon the Velocity with which it is traversed by Light.* By M. H. FIZEAU[*].

MANY theories have been proposed with a view of accounting for the phænomenon of the aberration of light according to the undulatory theory. In the first instance Fresnel, and more recently Doppler, Stokes, Challis, and several others have published important researches on this subject; though none of the theories hitherto proposed appear to have received the complete approval of physicists. Of the several hypotheses which have been necessitated by the absence of any definite idea of the properties of luminiferous æther, and of its relations to ponderable matter, not one can be considered as established; they merely possess different degrees of probability.

On the whole these hypotheses may be reduced to the following three, having reference to the state in which the æther ought to be considered as existing in the interior of a transparent body. Either, *first*, the æther adheres or is fixed to the molecules of the body, and consequently shares all the motions of the body; or *secondly*, the æther is free and independent, and consequently is not carried with the body in its movements; or, *thirdly*, only a portion of the æther is free, the rest being fixed to the molecules of the body and, alone, sharing its movements.

The last hypothesis was proposed by Fresnel, in order at once

[*] Translated from the *Annales de Chimie et de Physique* for December 1859. The original memoir was presented to the Parisian Academy of Sciences, Sept. 29, 1851; and a translation of the brief abstract published in the *Comptes Rendus* was given in the Phil. Mag. for December 1851, p. 568.

to satisfy the conditions of the aberration of light and of a celebrated experiment of Arago's, which proved that the motion of the earth does not affect the value of the refraction suffered by the light of a star on passing through a prism. Although these two phænomena may be explained with admirable precision by means of this hypothesis, still it is far from being considered at present as an established truth, and the relations between æther and matter are still considered, by most, as unknown. The mechanical conception of Fresnel has been regarded by some as too extraordinary to be admitted without direct proofs; others consider that the observed phænomena may also be satisfied by one of the other hypotheses; and others, again, hold that certain consequences of the hypothesis in question are at variance with experiment.

The following considerations led me to attempt an experiment the result of which promised, I thought, to throw light on the question.

It will be observed that, according to the first hypothesis, the velocity with which light traverses a body must vary with the motion of that body. If the motions of the body and the ray are like-directed, the velocity of light ought to be increased by the whole velocity of the body.

If the æther be perfectly free, the velocity of light ought not to be altered by the motion of the body.

Lastly, if the body when moving only carries with it a portion of the æther, then the velocity of light ought to be increased by a fractional part of the velocity of the body and not by the whole velocity, as in the first case. This consequence is not as evident as the two preceding ones, though Fresnel has shown that it is supported by mechanical considerations of a very probable nature.

The question then resolves itself to that of determining with accuracy the effect of the motion of a body upon the velocity with which light traverses it.

It is true that the velocity with which light is propagated is so immensely superior to any we are able to impart to a body, that any change in the first velocity must in general be inappreciable. Nevertheless, by combining the most favourable circumstances, it appeared to be possible to submit to a decisive test at least two media, air and water, to which, on account of the mobility of their particles, a great velocity may be imparted.

We owe to Arago a method of observation, founded on the phænomena of interference, which is well suited to render evident the smallest variation in the index of refraction of a body, and hence also the least change in the velocity with which the body is traversed by light; for, as is well known, this velocity is in-

versely proportional to the refracting index. Arago and Fresnel have both shown the extraordinary sensitiveness of this method by several very delicate observations, such as that on the difference of refraction between dry and moist air.

A method of observation founded upon this principle appeared to me to be the only one capable of rendering evident any change of velocity due to motion. It consists in obtaining interference bands by means of two rays of light after their passage through two parallel tubes, through which air or water can be made to flow with great velocity in opposite directions. The especial object before me necessitated several new arrangements, which I proceed to indicate.

With respect to the intensity of light, formidable difficulties had necessarily to be encountered. The tubes, which were of glass and 5·3 millims. in diameter, had to be traversed by light along their centres, and not near their sides; the two slits, therefore, had to be placed much further apart than is ordinarily the case, on which account the light would, in the absence of a special contrivance, have been very feeble at the point where the interference bands are produced.

This inconvenience was made to disappear by placing a convergent lens behind the two slits; the bands were then observed at the point of concourse of the two rays, where the intensity of light was very considerable.

The length of the tubes being tolerably great, 1·487 metre, it was to be feared that some difference of temperature or pressure between the two tubes might give rise to a considerable displacement of the bands, and thus completely mask the displacement due to motion.

This difficulty was avoided by causing the two rays to return towards the tubes by means of a telescope carrying a mirror at its focus. In this manner each ray is obliged to traverse the two tubes successively, so that the two rays having travelled over exactly the same path, but in opposite directions, any effect due to difference of pressure or temperature must necessarily be eliminated by compensation. By means of various tests I assured myself that this compensation was complete, and that whatever change in the temperature or density of the medium might be produced in a single tube, the bands would preserve exactly the same position. According to this arrangement, the bands had to be observed at the point of departure itself of the rays: solar light was admitted laterally, and was directed towards the tubes by means of reflexion from a transparent mirror; after their double journey through the tubes, the rays returned and traversed the mirror before reaching the place of interference, where the bands were observed by means of a graduated eye-piece.

The double journey performed by the rays had also the advantage of increasing the probable effect of motion; for this effect must be the same as if the tubes had double the length and were only traversed once.

This arrangement also permitted the employment of a very simple method for rendering the bands broader than they would otherwise have been in consequence of the great distance (9 millims.) between the slits. This method consisted in placing a very thick plate of glass before one of the slits, and inclining the same in such a manner that, by the effect of refraction, the two slits had the appearance of being very close to each other: in this manner the bands become as broad as they would be if the two slits were, in reality, as near each other as they appear to be; and instead of the intensity of light being sensibly diminished by this expedient, it may, in fact, be greatly augmented by giving greater breadth to the source of light. By causing the inclination of the glass to vary, the breadth of the bands may be varied at pleasure, and thus the magnitude most convenient for precisely observing their displacement may be readily given to them.

I proceed to describe the disposition of the tubes, and the apparatus destined to put the water in motion.

The two tubes, placed side by side, were closed at each extremity by a single glass plate, fixed with gum-lac in a position exactly perpendicular to their common direction. Near each extremity was a branch tube, forming a rounded elbow, which established a communication with a broader tube reaching to the bottom of a flask; there were thus four flasks communicating with the four extremities of the tubes.

Into one flask, which we will suppose to be full of water, compressed air, borrowed from a reservoir furnished with an air-pump, was introduced through a communicating tube. Under the influence of this pressure the water rose from the flask into the tube, which it then traversed in order to enter the flask at the opposite end. The latter could also receive compressed air, and then the liquid returned into the first flask after traversing the tube in an opposite direction. In this manner a current of water was obtained whose velocity exceeded 7 metres per second. A similar current, but in an opposite direction, was produced at the same time in the other tube.

Within the observer's reach were two cocks fixed to the reservoir of air; on opening either, currents, opposite in direction, were established in both tubes; on opening the other cock the currents in each tube were simultaneously reversed.

The capacity of the reservoir, containing air at a pressure of about two atmospheres, amounted to 15 litres (half a cubic foot), that of each flask to about 2 litres; the latter were divided into

equal volumes, and the velocity of the water was deduced from the section of the tubes, and from the time of efflux of half a litre.

The apparatus above described was only employed for the experiments with water in motion: with some modifications it might also be used for air; but my experiments on moving air had been previously made with a slightly different apparatus, of which more hereafter, and the results had been found quite conclusive. I had already proved that *the motion of air produces no appreciable displacement of the bands.* But I shall return to this result and give further details.

For water there is an evident displacement. *The bands are displaced towards the right when the water recedes from the observer in the tube at his right, and approaches him in the tube on his left.*

The displacement of the bands is towards the left when the direction of the current in each tube is opposite to that just defined.

During the motion of the water the bands remain well defined, and move parallel to themselves, without the least disorder, through a space apparently proportional to the velocity of the water. With a velocity of 2 metres per second even, the displacement is perceptible; for velocities between 4 and 7 metres it is perfectly measureable.

In one experiment, where a band occupied five divisions of the micrometer, the displacement amounted to 1·2 divisions towards the right and 1·2 divisions towards the left, the velocity of the water being 7·059 metres per second. The sum of the two displacements, therefore, was equal to 2·4 divisions, or nearly half the breadth of a band.

In anticipation of a probable objection, I ought to state that the system of the two tubes and four flasks, in which the motion of the water took place, was quite isolated from the other parts of the apparatus : this precaution was taken in order to prevent the pressure and shock of the water from producing any accidental flexion in parts of the apparatus whose motion might influence the position of the bands. I assured myself, however, that no such influence was exerted, by intentionally imparting motions to the system of the two tubes.

After establishing the existence of the phænomenon of displacement, I endeavoured to estimate its magnitude with all possible exactitude. To avoid all possible sources of error, I varied the magnification of the bands, the velocity of the water, and even the nature of the divisions of the micrometer, so as to be unable to predict the magnitude of the displacements before measuring them. For in measuring small quantities, where our own power of estimating has to play a great part, the influence of any preconception is always to be feared; I think, however,

that the result I have obtained is altogether free from this cause
of error.

For the most part the observations were made with a velocity
of 7·059 metres per second; in a certain number the velocity
was 5·515 metres, and in others 3·7 metres.   The magnitudes
observed have been all reduced to the maximum velocity 7·059
metres, and referred to the breadth of a band as unity.

Displacements of the bands for a mean velocity of water equal to 7·059 metres per second.	Differences between the observed displacements and their mean value.
0·200	−0·030
0·220	−0·010
0·240	+0·010
0·167	−0·063
0·171	−0·059
0·225	−0·005
0·247	+0·017
0·225	−0·005
0·214	−0·016
0·230	0·000
0·224	−0·006
0·247	+0·017
0·224	−0·006
0·307	+0·077
0·307	+0·077
0·256	+0·026
0·240	+0·010
0·240	+0·010
0·189	−0·041

Sum   .   4·373

Mean   .   0·23016

By doubling the mean value we have 0·46, nearly half the
breadth of a band, which represents the magnitude of the dis-
placement produced by reversing the direction of the current in
each tube.

To show the deviations on each side, the differences between
the several observed displacements and the mean value of all
have been inserted in the Table.   It will be seen that, in general,
they represent a very small fraction of the breadth of a band;
the greatest deviation does not exceed one-thirteenth of the
breadth of a band.

These differences are due to a difficulty which could not be
overcome; the displacement remained at its maximum but for a
very short period, so that the observations had to be made very

rapidly. Had it been possible to maintain the velocity of the current of water constant for a greater length of time, the measurements would have been more precise; but this did not appear to be possible without considerably altering the apparatus, and such alterations would have retarded the prosecution of my research until the season was no longer favourable for experiments requiring solar light.

I proceed to compare the observed displacement with those which would result from the first and third hypotheses before alluded to. As to the second hypothesis, it may be at once rejected; for the very existence of displacements produced by the motion of water is incompatible with the supposition of an æther perfectly free and independent of the motion of bodies.

In order to calculate the displacement of the bands under the supposition that the æther is united to the molecules of bodies in such a manner as to partake of their movements, let

v be the velocity of light in a vacuum,

v' the velocity of light in water when at rest,

u the velocity of the water supposed to be moving in a direction parallel to that of the light. It follows that

$v' + u$ is the velocity of light when the ray and the water move in the same direction, and

$v' - u$ when they move in opposite directions.

If Δ be the required retardation and E the length of the column of water traversed by each ray, we have, according to the principles proved in the theory of the interference of light,

$$\Delta = E\left(\frac{v}{v' - u} - \frac{v}{v' + u}\right),$$

or

$$\Delta = 2E\frac{u}{v} \cdot \frac{v^2}{v'^2 - u^2}.$$

Since u is only the thirty-three millionth part of v, this expression may, without appreciable error, be reduced to

$$\Delta = 2E\frac{u}{v} \cdot \frac{v^2}{v'^2}.$$

If $m = \frac{v}{v'}$ be the index of refraction of water, we have the approximate formula

$$\Delta = 2E\frac{u}{v} m^2.$$

Since each ray traverses the tubes twice, the length E is double the real length of the tubes. Calling the latter L = 1·4875 metre,

the preceding formula becomes

$$\Delta = 4L \frac{u}{v} m^2;$$

and the numerical calculation being performed, we find

$$\Delta = 0 \cdot 0002418 \text{ millim.}$$

Such is the difference of path which, under the present hypothesis, ought to exist between the two rays.

Strictly speaking, this number has reference to a vacuum, and ought to be divided by the index of refraction for air; but this index differs so little from unity, that, for the sake of simplicity, the correction, which would not alter the last figure by a unit, may be neglected.

The above quantity being divided by the length of an undulation, will give the displacement of the bands in terms of the breadth of one of them.   In fact, for a difference of path amounting to 1, 2, ... *m* undulations, the system of bands suffer a displacement equal to the breadth of 1, 2, ... *m* bands.

For the ray E the length of an undulation is $\lambda = 0 \cdot 000526$, and the rays about it appear to preserve the greatest intensity after the light has traversed a rather considerable thickness of water.   Selecting this ray, then, we find for the displacement the value

$$\frac{\Delta}{\lambda} = 0 \cdot 4597.$$

Had, therefore, the æther participated fully in the motion of the water, in accordance with the hypothesis under consideration, a displacement of $0 \cdot 46$ of a band would have been observed in the foregoing experiments.   But the mean of our observations gave only $0 \cdot 23$; and on examining the greatest particular values, it will be found that none approached the number $0 \cdot 46$.   I may even remark that the latter number ought to be still greater, in consequence of a small error committed in the determination of the velocity of the water; an error whose tendency is known, although, as will soon be seen, it was impossible to correct it perfectly.

I conclude, then, that this hypothesis does not agree with experiment.   We shall next see that, on the contrary, the third, or Fresnel's hypothesis, leads to a value of the displacement which differs very little from the result of observation.

We know that the ordinary phænomena of refraction are due to the fact that light is propagated with less velocity in the interior of a body than in a vacuum.   Fresnel supposes that this change of velocity occurs because the density of the æther within a body is greater than that in a vacuum.   Now for two media

whose elasticity is the same, and which differ only in their densities, the squares of the velocities of propagation are inversely proportional to these densities; that is,

$$\frac{D'}{D} = \frac{v^2}{v'^2},$$

D and D′ being the densities of the æther in a vacuum and in the body, and v, v' the corresponding velocities. From the above we easily deduce the relations

$$D' = D\frac{v^2}{v'^2}, \quad D' - D = D\frac{v^2 - v'^2}{v'^2},$$

the latter of which gives the excess of density of the interior æther.

It is assumed that when the body is put in motion, only a part of the interior æther is carried along with it, and that this part is that which causes the excess in the density of the interior over that of the surrounding æther; so that the density of this moveable part is D′−D. The other part which remains at rest during the body's motion has the density D.

The question now arises, With what velocity will the waves be propagated in a medium thus constituted of an immoveable and a moveable part, when for the sake of simplicity we suppose the body to be moving in the direction of the propagation of the waves?

Fresnel considers that the velocity with which the waves are propagated then becomes increased by the velocity of the centre of gravity of the stationary and moving portions of æther. Now u being the velocity of the body,

$$\frac{D' - D}{D'} u$$

will be the velocity of the centre of gravity of the system in question, and according to the last formula this expression is equal to

$$\frac{v^2 - v'^2}{v^2} u.$$

Such, then, is the quantity by which the velocity of light will be augmented; and since v' is the velocity when the body is at rest,

$$v' + \frac{v^2 - v'^2}{v^2} u \text{ and } v' - \frac{v^2 - v'^2}{v^2} u$$

will be the respective velocities when the body moves with and against the light.

By means of these expressions the corresponding displacement of the bands in our experiment may be calculated in exactly the

same manner as before.   For the difference of path we have the value

$$\Delta = \mathrm{E} \left\{ \frac{v}{v' - \dfrac{v^2 - v'^2}{v^2}u} - \frac{v}{v' + \dfrac{v^3 - v'^2}{v^2}u} \right\},$$

which by reduction and transformation becomes

$$\Delta = 2\mathrm{E}\frac{u}{v} \left\{ \frac{v^2 - v'^2}{v'^2 - u^2 \left(\dfrac{v^2 - v'^2}{v^2}\right)^2} \right\}.$$

Taking into consideration the smallness of u with respect to $v' \left(\dfrac{u}{v'} = \dfrac{1}{33000000} \right)$, and the circumstance that the coefficient of u^2 differs little from unity, the term in u^2 may, without appreciable error, be neglected, and the above expression considerably simplified.   In fact, if m be the index of refraction, and $\mathrm{L} = \frac{1}{2}\mathrm{E}$ the length of each tube, we have approximately

$$\Delta = 4\mathrm{L}\frac{u}{v}(m^2 - 1),$$

whence by numerical calculation we deduce

$$\Delta = 0 \cdot 00010634 \text{ millim.}$$

On dividing this difference of path by the length λ of an undulation, the magnitude of the displacement becomes

$$\frac{\Delta}{\lambda} = 0 \cdot 2022,$$

the observed value being 0·23.

These values are almost identical; and what is more, the difference between observation and calculation may be accounted for with great probability by the presence of the before-mentioned error in estimating the velocity of the water.   I proceed to show that the tendency of this error may be assigned, and that analogy permits us to assume that its effect must be very small.

The velocity of the water in each tube was calculated by dividing the volume of water which issued per second from one of the flasks by the sectional area of the tube.   But by this method it is only the *mean* velocity of the water which is determined; in other words, that which would exist provided the several threads of liquid at the centre and near the sides of the tube moved with equal rapidity.   It is evident, however, that this cannot be the case; for the resistance opposed by the sides of the tube, acting in a more immediate manner on the neighbouring threads of liquid, tends to diminish their velocity more than it does that of the threads nearer the centre of the tube.   The velocity of the

water in the centre of the tubes, therefore, must be greater than that of the water near the sides, and consequently also greater than the mean of both velocities.

Now the slits placed before each tube to admit the rays whose interference was observed, were situated in the middle of the circular ends of the tubes; so that the rays necessarily traversed the central zones, where the velocity of the water exceeded the mean velocity*.

The law followed by these variations of velocity in the motion of water through tubes not having been determined, it was not possible to introduce the necessary corrections. Nevertheless analogy indicates that the error resulting therefrom cannot be considerable.   In fact, this law has been determined in the case of water moving through open canals, where the same cause produces a similar effect; the velocity in the middle of the canal and near the surface of the water is there also greater than the mean velocity.   It has been found that, for values of the mean velocity included between 1 and 5 metres per second, the maximum velocity is obtained by multiplying this mean velocity by a certain coefficient which varies from 1·23 to 1·11.   Analogy therefore permits us to assume that in our case the correction to be introduced would be of the same order of magnitude.

Now on multiplying u by 1·1, 1·15, and 1·2, and calculating the corresponding values of the displacement of the bands, we find in place of 0·20 the values 0·22, 0·23, 0·24 respectively; whence it will be seen that in all probability the correction would tend to cause still greater agreement between the observed and the calculated results.   We may presume, then, that the small difference which exists between the two values depends upon a small error in estimating the real velocity of the water; which error cannot be rectified in a satisfactory manner, in consequence of the absence of sufficiently accurate data.

Thus the displacement of the bands caused by the motion of water, as well as the magnitude of this displacement, may be explained in a satisfactory manner by means of the theory of Fresnel.

It was before observed that the motion of air causes no perceptible displacement of the bands produced by the interference of two rays which have traversed the moving air in opposite directions.   This fact was established by means of an apparatus which I will briefly describe.

A pair of bellows, loaded with weights and worked by a lever, impelled air forcibly through two parallel copper tubes whose extremities were closed by glass plates.   The diameter of each

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* Each slit was a rectangle 3 millims. by 1·5, and its surface was equal to one-fifth that of the tube.

tube was 1 centimetre, and its effective length 1·495 metre; the direction of the motion in one tube was opposite to that in the other, and the pressure under which this motion took place was measured by a manometer placed at the entrance of the tubes; it could be raised to 3 centimetres of mercury.

The velocity of the air was deduced from the pressure and from the dimensions of the tubes, according to the known laws of the efflux of gases.   The value thus found was checked by means of the known volume of the bellows, and the number of strokes necessary to produce a practically constant pressure at the entrance of the tubes.   A velocity of 25 metres per second could easily be imparted to the air; occasionally greater velocities were reached, but their values remained uncertain.

In no experiment could a perceptible displacement of the bands be produced : they always occupied the same positions, no matter whether the air remained at rest, or moved with a velocity equal or even superior to 25 metres per second.

When this experiment was made, the possibility of doubling, by means of a reflecting telescope, the value of the displacement, and at the same time of completely compensating any effects due to accidental differences of temperature or pressure in the two tubes, had not suggested itself; but I employed a sure method of distinguishing between the effects due to motion, and those resulting from accidental circumstances.

This method consisted in making two successive observations, by causing the rays to traverse the apparatus in opposite directions.   For this purpose the source of light was placed at the point where the central band had previously been, when the new bands formed themselves where the source of light had previously been placed.

The direction of the motion of the air in the tubes remaining the same in both cases, it is easy to see that the accidental effects would in both observations give rise to a displacement towards the same tube, whilst the displacement due solely to motion would first be on the side of one tube and then on the side of the other.   In this manner a displacement due to motion would have been detected with certainty, even if it had been accompanied by an accidental displacement due, for instance, to some defect of symmetry in the diameters or orifices of the tubes, whence would result an unequal resistance to the passage of air, and consequently a difference of density.

But the symmetry given to the apparatus was so perfect that no sensible difference of density existed in the two tubes during the motion of the air.   The double observation was consequently unnecessary; nevertheless it was made for the sake of greater security, and in order to be sure that the sought displacement

was not accidentally compensated by a difference of density, which, though small, might be sufficient totally to mask such displacement.

Notwithstanding these precautions, however, no displacement of the bands occurred in consequence of the motion of the air; and according to an estimate I have made, a displacement equal to one-tenth of the breadth of a band would have been detected had it occurred.

The calculations with respect to this experiment are as follows. Under the hypothesis that the air, when moving, carries with it all the æther, we have

$$\Delta = 2\mathrm{L}\frac{u}{v}\,m^2 = 0 \cdot 0002413 \text{ millim.},$$

m^2 being equal to $1 \cdot 000567$ at the temperature 10° C.

This experiment having been made in air, the maximum illumination was due to the yellow rays; and this maximum determined the breadth of the bands. Hence the value of λ corresponding to the ray D being taken, we have

$$\frac{\Delta}{\lambda} = 0 \cdot 4103.$$

Now so great a displacement could certainly not have escaped observation, especially since it might have been doubled by reversing the current.

The following would be the results of the calculation according to the hypothesis of Fresnel :—

$$\Delta = 2\mathrm{L}\frac{u}{v}\,(m^2 - 1) = 0 \cdot 0000001367,$$

$$\frac{\Delta}{\lambda} = 0 \cdot 0002325.$$

Now a displacement equal to $\frac{1}{5000}$th of the breadth of a band could not be observed; it might, in fact, be a hundred times greater and still escape observation. Thus the apparent immobility of the bands in the experiment made with moving air may be explained by the theory of Fresnel, according to which the displacement in question, although not absolutely zero, is so small as to escape observation.

After having established this negative fact, and seeking, by means of the several hypotheses respecting æther, to explain it as well as the phænomenon of aberration and the experiment of Arago, it appeared to me to be necessary to admit, with Fresnel, that the motion of bodies changes the velocity with which light traverses them, but that this change of velocity varies according to the energy with which the traversed medium refracts light; so

that the change is great for highly refracting bodies, but small for feebly refracting ones such as air.

I was thus led to anticipate a sensible displacement of the bands by means of the motion of water, since its index of refraction greatly exceeds that of air.

It is true that an experiment of Babinet's, mentioned in the ninth volume of the *Comptes Rendus*, appeared to be in contradiction to the hypothesis of a change in the velocity of light in accordance with the law of Fresnel. But on considering the conditions of that experiment, I detected the existence of a cause of compensation whose influence would render the effect due to motion insensible. This cause proceeds from the reflexion which the light suffers in the experiment in question. It may, in fact, be demonstrated that if a certain difference of path exists between two rays, that difference becomes altered when these rays suffer reflexion from a moving mirror. Now on calculating separately the two effects (of reflexion) in the experiment of Babinet, their magnitudes will be found to be equal and opposite in sign.

This explanation rendered the hypothesis of a change of velocity still more probable, and induced me to undertake the experiment with water, as being the most suitable one for deciding the question with certainty.

The success of this experiment must, I think, lead to the adoption of the hypothesis of Fresnel, or at least to that of the law discovered by him, which expresses the relation between the change of velocity and the motion of the body; for although the fact of this law being found to be true constitutes a strong argument in favour of the hypothesis of which it is a mere consequence, yet to many the conception of Fresnel will doubtless still appear both extraordinary and, in some respects, improbable; and before it can be accepted as the expression of the real state of things, additional proofs will be demanded from the physicist, as well as a thorough examination of the subject from the mathematician.

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Shortly before the publication of the above interesting memoir in the *Annales de Chimie*, M. Fizeau presented to the Academy a second memoir, containing the results of his experiments on the effect of the motion of a transparent solid body, such as glass, upon the velocity with which it is traversed by light. The *Comptes Rendus* of November 14th, 1859, contains a brief extract from this memoir; and from it we gather the principal results of his experiments, and the principles upon which the same were based.

The method of experiment which was employed in the fore-

going researches on air and water being no longer applicable, recourse was had to the following property of light established by the researches of Malus, Biot, and Brewster. When a ray of polarized light traverses a plate of glass, inclined towards its direction, the plane of polarization of the transmitted ray is in general inclined towards that of the incident ray. The magnitude of the rotation of the plane of polarization which is thus caused by the two refractions at the two surfaces of the plate of glass depends, *first*, upon the angle of incidence; *secondly*, upon the azimuth of the primitive plane of polarization with reference to the plane of incidence; and *thirdly*, upon the index of refraction of the glass forming the plate.

The angle of incidence and the azimuth of the primitive plane of polarization remaining the same, the rotation of this plane increases with the index of refraction of the glass plate. Now since this index is inversely proportional to the velocity with which waves of light are propagated through the glass, it follows that the magnitude of the rotation of the plane of polarization increases when the velocity with which light traverses the glass plate diminishes. The determination of any change in this velocity is, therefore, reduced to that of the corresponding change in the rotation of the plane of polarization.

In the first place it was deemed necessary to determine the change in the rotation which any given increase or decrease of the index of refraction could produce. By direct and comparative measurements of these indices and rotations, in the cases of flint and ordinary glass, it was found that when the index was increased by a small fraction, the rotation increased by a fraction $4\frac{1}{2}$ times greater than the first.

The question next arises what change, according to the hypothesis of Fresnel, ought to be produced in the velocity of light when it traverses glass in a state of motion? The answer is based upon the following data.

The greatest velocity at our command is unquestionably that of the earth in its orbit. At noon, during the period of the solstices, for instance, the direction of this motion is horizontal and from east to west; from this it follows that when a plate of glass receives a ray of light coming from the west, it ought to be considered as really moving to meet the ray with the immense velocity of 31,000 metres per second. When, on the contrary, the incident ray comes from the east, the glass plate must be considered as moving with this velocity in the same direction as that of the propagation of the waves of light, by which latter it is in reality overtaken.

Now, according to the theory of Fresnel, the difference between the velocities of the light in these two extreme cases would be

sufficient to produce a change in the rotation of the plane of polarization equal to $\frac{1}{1500}$th part of the magnitude of that rotation.

In order to test this result by experiment, a series of glass plates were interposed in the path of a polarized beam of parallel rays of light. The primitive plane of polarization was determined by a divided circle, and the rotation which this plane underwent by the action of the plates was measured by means of a second graduated circle fixed to a convenient analyser. The instrument could, moreover, be fixed in any direction so as to study the influence of all terrestrial motions upon the phænomena.

In order to make the two necessary observations conveniently and rapidly, two mirrors were previously fixed on the east and on the west of the instrument, and upon each, alternately, a beam of solar light was thrown by means of a heliostat, and thence reflected towards the instrument.

The greatest difficulties were encountered in the annealing of the glass plates of the series ; and as perfectly homogeneous plates could not be obtained, it was necessary to employ various compensating expedients, all which will be found described in the memoir itself.

The conclusions to which M. Fizeau was led by means of more than 2000 observations are thus stated :— :

1. The rotation of the plane of polarization produced by a series of inclined glass plates is always greater when the light which traverses them comes from the west than when it comes from the east ; the observation being made about noon.

2. This excess of rotation is decidedly at a maximum at or about noon during the solstices. Before and after this hour it is less, and at about 4 o'clock is scarcely perceptible.

3. The numerical values deduced from the numerous series of observations present notable differences, the cause of which may be guessed, though it cannot yet be determined with certitude.

4. The influence of the earth's annual motion, as determined by calculation on the hypothesis of Fresnel, leads to values of the above excess of rotation which agree tolerably well with the majority of the values deduced from observation.

5. Theory, as well as experiment, therefore, lead us to conclude that the azimuth of the plane of polarization of a refracted ray is really influenced by the motion of the refracting medium, and that the motion of the earth in space exerts an influence of this kind upon the rotation of the plane of polarization produced by a series of inclined glass plates.

XXXIII. *On a new Instrument for the Mechanical Trisection of an Angle; and on the Multisection of an Angle. By* Thomas Tate, *Esq.**

THE trisection of an angle is a subject of historical celebrity, and, apart from its utility, must always be interesting to the mathematician. Some years ago, Professor Christie invented an instrument for the mechanical trisection of an angle, which consists of four rods, kept at equal angles apart from each other by means of linkwork. Although simple in principle, this instrument is somewhat complicated in construction, and therefore necessarily, to some extent, inaccurate as regards its application. This instrument contains four rods, ten links, ten axes, and two sliding-pieces; whereas the instrument which I have made contains only two rods, four links, four axes, and two sliding-pieces. Both instruments are mathematically correct in principle.

The instrument which I have constructed is represented in the annexed diagram. A B and A C are two rods turning on the axis A; D E, D F, D H, and D G are four links, each equal in length to A E or A F, turning on a common axis D, D E being connected with A B by an axis at E, and D F with A C by an axis at F. The pin G, of the link D G, slides in the slit *a b* formed in the rod A B in the line of the axes A and E; and the pin H, of the link D H, slides in the slit *e c* formed in the rod A C in the line of the axes A and F. The inner edges of the four links, D E, D F, D H, D G, are in a line with the centres of their respective axes, as shown in the diagram. The rods A B and A C are connected by a half-lap joint at A; and similarly the four links are connected at D by half-lap joints, so that the pieces all lie flat upon the surface
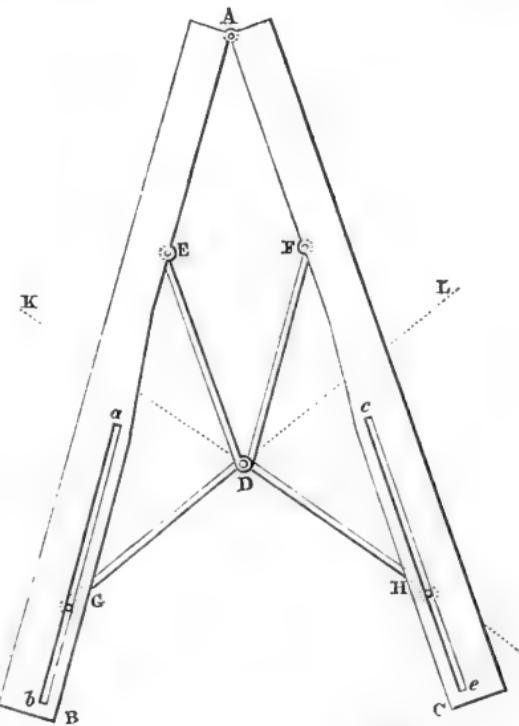
* Communicated by the Author.

of the paper. The links D E, D F, D H, D G lie below the rods A B and A C at their respective points of connexion, so that the plane of A B C always lies parallel to the plane of the paper. The instrument is used in the following manner.

Let K D L be the angle to be trisected. Produce K D to H, and L D to G. Laying hold of the extremities B and C of the rods A B and A C, move the links D H and D G until their inner edges coincide with the lines forming the angle G D H; draw lines D E, D F along the inner edges of the links D E and D F; then these lines will trisect the given angle K D L, as required.

Demonstration.—In all positions of the instrument, AEDF is an equilateral parallelogram, and DGE, DHF are equal isosceles triangles.

Because DG = DE, ∠ GED = ∠ DGE; and because DF is parallel to EG, ∠ LDF = ∠ DGE; therefore ∠ LDF = ∠ GED; but because DF is parallel to EG, ∠ EDF = ∠ GED; therefore ∠ LDF = ∠ EDF.

In like manner it may be shown that ∠ KDE = ∠ EDF; therefore the lines DF and DE trisect the angle KDL.

I have made this instrument of lance-wood, with brass pivots. The links are each 8 inches long, $\frac{1}{4}$ of an inch in width, and $\frac{3}{16}$ths of an inch in thickness. By means of this instrument, any angle not much exceeding two right angles and not less than nine degrees, may be at once trisected with great precision; but by an obvious mathematical artifice it may be used for the trisection of angles, however small or large they may be.

It is scarcely necessary to observe, that all general methods for the multisection of a given arc can only be approximate. In such cases there can be no objection to the use of approximate methods of construction, provided that they are given as such. By the following method of construction, a given arc may be divided into any number of equal parts, with an approach to truth which is only limited by the inac-

curacies necessarily involved in the drawing of lines through points formed by the intersection of straight lines or circular arcs.
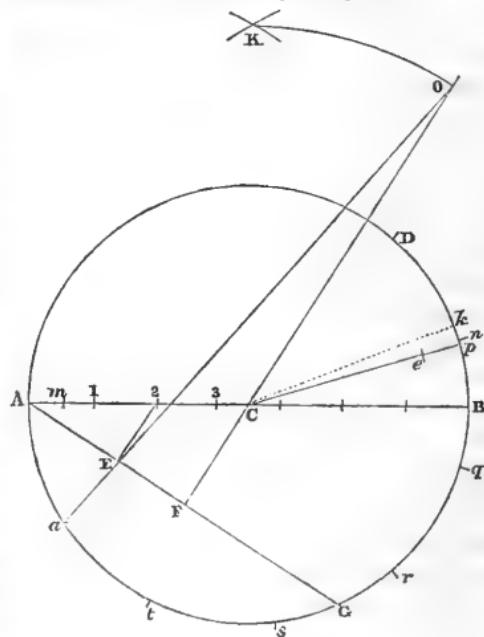
Let it be required to divide the given arc A B D, whose centre is C, into any number of equal parts. (In the example here given, the arc is divided into seven equal parts.)

First approximation.—Bisect the given arc in G, and draw the chord A G. Through A draw the diameter ACB, and divide it into *n* equal parts in the points 1, 2, 3, &c. Bisect the chord A G by the perpendicular F C O; from A and B as centres, with the diameter A B as radius, describe arcs cutting each other in K; and from the centre C, with C K as a radius, describe the arc K O, cutting F C O in O. Through the point 2 draw 2 E parallel to O F, meeting A G in E; and through E and O draw the straight line O E *a*, meeting the arc in *a*; then A *a* will be approximately the *n*th part of the arc A G B D, but which may be determined with greater precision as follows.

Second approximation.—Take off the chord A *a* in the compasses, and apply it on the arc A G D proceeding from *a* towards D; let *k* be the last point in the division; apply this chord from D to *p*, and join *p* C. Bisect A 1 in *m*; from the centre C, with C *m* as a radius, describe an arc cutting C *p* in *e*; through *e* draw *e n* parallel to C *k*, cutting the arc in the point *n*; then D *n* will be practically the *n*th part of the given arc A G B D, as required.

When the number of parts into which the given arc is to be divided is considerable, especially when the arc is equal to or nearly equal to the whole circumference of the circle, the second operation in the foregoing process becomes necessary in order to attain a sufficient degree of accuracy.

The proposition contained in the first approximation is a generalization of a well-known method, sometimes employed by practical men, for the division of the whole circumference of the circle into a given number of equal parts.

Hastings, March 6, 1860.

XXXIV. *On certain Laws of Chromatic Dispersion.*
By Mungo Ponton, *F.R.S.E.*

[Continued from p. 181.]

WITH a view to an examination of the results arrived at in the previous part of this paper, it will be found convenient to classify the observations. Fraunhofer has fortunately given two sets of observations for water, and also for flint-glass No. 23; and from these a judgment may be formed of the degree of accuracy attainable. It will be found that, while

the two sets agree very closely, this agreement does not extend beyond the fourth place of decimals; so that in no case can the fifth and sixth places, as given by observation, be depended on, while it is needful to carry the indices to the sixth place of decimals in order to their fulfilling with exactness the exponential law. But if the observed indices be correct down to the fourth place of decimals, the exponential law, in combination with the laws governing the extrusions, may be relied on for the fifth and sixth places.

It is proposed, then, to consider all those observations in which, when tested by these laws, no individual error amounts to 0·0001, as of the first order, greater accuracy of observation being unattainable; those in which no individual error amounts to 0·0002, as of the second order; those in which no individual error amounts to 0·0003, as of the third order, and so on.

As regards Fraunhofer's observations, it will be found that, of the whole twelve, there are of the first order seven; namely, water (two sets), solution of potash, oil of turpentine, and three specimens of crown-glass; while there are of the second order five, all of them on flint-glass.

With respect to Rudberg's ten observations on doubly-refracting media, there are of the first order seven, namely, topaz 2nd axis, quartz ex. ray; Arragonite 1st axis, quartz O. ray; topaz 3rd axis, calc-spar O. ray; topaz 1st axis: and of the second order three, namely, Arragonite 3rd axis, calc-spar ex. ray, and Arragonite 2nd axis.

With Powell's observations the results are not so satisfactory; but that the discrepancies which they present are due, not to any peculiarity in the media, nor to any defect in the exponential law, but simply to the inaccuracy of the observations themselves, may be clearly shown. It fortunately happens that we have a set of observations by Powell on water, on which we have two sets by Fraunhofer. While the two latter agree very closely with each other, and quite as closely with the exponential law, both being of the first order, those of Powell are so inaccurate that they can be classed only as of the eighth order. Now this difference can be attributed to no other cause whatever than to the inferior accuracy of Powell's observations; for it appears highly improbable that it should be due to the difference of temperature at which the observations were made. In the case of solution of potash, on which we have also observations both by Fraunhofer and Powell, and at temperatures more widely apart, the difference in quality between the two sets is much less marked; for while those of Fraunhofer are of the first order, those of Powell are of the second.

But if in so simple a case as that of water the observations of

Powell are so very inferior to those of Fraunhofer, it should not occasion surprise to find that in some instances this inferiority is still more marked, and that some of Powell's observations, when tested by the exponential law, should be classed as only of the tenth order. Of this we have an example in the case of nitrate of potash, which is of low dispersive power, and should therefore have presented no peculiar difficulties. But this case belongs to a class of observations of which the observer himself says, that the media being of low dispersive power, and considered by him of little importance, the calculations were in consequence carried to only a slight degree of approximation. So far, then, from the discrepancies, in the case of nitrate of potash, tending to shake confidence in the exponential law of the indices, they ought to be regarded as strengthening its probability, by showing it to be capable of detecting the errors in these observations which might otherwise have escaped notice, seeing that, in conformity with the observed indices, nitrate of potash belongs to the class of regular media; and there is no other test by which the errors could have been brought to light.

This example shows that, with reference to any general law of dispersion, no medium ought to be viewed as of small importance, but that the same attention should be given to secure accuracy in media of low, as in those of high dispersive power.

Of the twenty-nine observations by Powell which have been tested by the exponential law of the indices, there is not one that can be classed as of the first order—a fact sufficiently indicative of their general inferiority. There are, however, thirteen, or nearly one-half, which are of the second order, and may therefore be regarded as fair observations. These are—

Sulphate of magnesia.	Nitrate of bismuth.	Superacetate of lead.
Solution of potash.	Nitrate of lead.	Nitric acid.
Sulphate of soda.	Subacetate of lead.	Oil of sassafras.
Alcohol.	Muriate of ammonia.	Oil of anise, T. $13°\cdot25$.

and the same at temp. $20°\cdot9$. There are five of the third order, muriatic acid, nitrate of mercury, muriate of lime, rock-salt, and oil of anise, temp. $15°\cdot8$. There are two of the fourth order— sulphuric acid and creosote; three of the fifth order—pyroligneous acid, bisulphuret of carbon, and muriate of baryta; one of the sixth order—oil of cassia, temp. $14°$; two of the seventh order—solution of soda, and oil of cassia, temp. $10°$; one of the eighth order—water, temp. $15°\cdot8$; one of the ninth order —oil of cassia, temp. $22°\cdot5$; and one of the tenth order—solution of nitrate of potash. From this enumeration it is clear that there is no connexion between the amount of error and the dispersive power of the medium, seeing we have media of low

and high dispersive power indifferently among the best and the worst cases.

In the three sets of observations, there are fourteen of the first order, in which the agreement with the exponential law may be considered perfect; and there are twenty-one of the second order, in which the agreement may be regarded as nearly complete. These together amount to thirty-five out of the fifty-one, or better than two-thirds of the whole. To these may be added the five of the third order, in which the agreement may be considered fair, thus making four-fifths of the whole, in which the observed and calculated indices agree as nearly as can be reasonably expected.

The whole errors in Fraunhofer's twelve observations amount to 0·003449, or 0·000287 per medium, and 0·000041 per line. In Rudberg's ten observations, the sum total of the errors is 0·003204, or 0·000320 per medium, or 0·000046 per line, so that these two sets are nearly equal in quality. In Powell's twenty-nine observations, the total errors amount to 0·034400, or about 0·001180 per medium, and 0·000170 per line; so that, in general accuracy, Fraunhofer's observations are to Powell's nearly in the ratio of 4 to 1.

That the whole of the discrepancies between the observed indices and those calculated by the exponential law are due, not to any defect or inaccuracy in that law, but solely to inaccuracies in the observations, it is not difficult to show. As regards the fourteen observations of the first order, there can be no doubt whatever. With respect to those of the second order, it fortunately happens that the two sets of observations on oil of anise, at temp. 13°·25 and temp. 20°·9, are both of this order, and agree very nearly,—the cumulo errors in the former being 0·000393, and in the latter 0·000387. But the observations on the same medium, at the intermediate temperature 15°·1, are of only the third order,—the cumulo errors being 0·000748, about double of those in the former cases. Now this difference can arise from no other cause than a difference in the degree of accuracy with which the observations were made; so that there is here a difference in the amount of error, arising simply from an inferior degree of accuracy in the observations, equal to the total amount of error in the two best observations on oil of anise, thus showing that these latter errors must themselves be due to defective observation. But it is equally clear that the greater errors in the worst set must also arise from inaccurate observations; for had these been made with the same care as the two first, they would have been of the same quality. It may hence be fairly inferred, that in all the observations, thirty-seven in number, in which the cumulo differences do not exceed 0·000748, these are due to incorrect observations.

Among the fourteen media in which the discrepancies are greater, there is found water, as observed by Powell, in which the total errors amount to no less than 0·001916; whereas in Fraunhofer's two sets of observations on this medium, their amounts are 0·000154 and 0·000205,—Powell's discrepancies exceeding the least of Fraunhofer's by 0·001762, an excess which can be due to nothing but a difference in the degree of accuracy with which the observations were made. Thus the total discrepancies of 0·001916 in Powell's observations on water are clearly traceable to experimental inaccuracy. But the total discrepancies in the case of Powell's observations on oil of cassia, temp. 14°, very little exceed this amount, being 0·001984; so that these may also be fairly attributed to the same cause. Now the reasoning applicable in the case of the oil of anise applies equally to the observations on oil of cassia at temp. 10° and temp. 22°·5. The discrepancies in these two cases amount respectively to 0·003750 and 0·003529, or not far from double of what they are at the intermediate temperature 14°. This difference can be attributed to nothing but the inferior accuracy with which the observations at temp. 10° and temp. 22°·5 were made; and had only the same amount of care been bestowed on these as on those made at temp. 14°, the gross amount of discrepancies would not have exceeded those presented in the latter case, which have already been shown to be due to experimental error. Thus the extreme amount of the discrepancies in the case of oil of cassia, temp. 10°, may be logically traced to defective observation; and these discrepancies being the greatest in the Table, it may hence be quite fairly inferred that all those of lower amount ought to be attributed to the same cause.

The indices, calculated by the exponential law from the fourteen observations of the first order, may be regarded as being quite as correct as they can be possibly obtained. Those calculated from the twenty-one observations of the second order may be deemed very nearly correct; while those calculated from the five observations of the third order may be viewed as fair approximations to the truth. It is, however, too much to expect of the exponential law that it should yield *accurate* indices from the eleven observations of an order inferior to the third. No mathematical law whatever can bring forth accurate results from incorrect observations where the errors exceed a certain limit; the utmost that can be expected in such a case is, that the law should indicate the *probable* position and amount of the errors of observation, and exhibit the necessity for more careful repetition. It is in this light, then, that the calculated indices of these eleven cases ought to be regarded.

This point must be kept in view in examining the question,

how far the peculiarities observed in some of the media, as respects the number and position of the nodes of the extrusions, may be traceable to errors of observation.

In the case of sulphuric acid, these peculiarities are removed by the entire extinction of the extrusions, under the operation of the exponential law.   But the extrusions are brought to assume, by virtue of this law, the regular type in the following cases, in which the observed indices cause them to appear quite irregular, namely, muriatic acid, alcohol, solution of soda, pyroligneous acid, and oil of anise, temp. 15°.   See end of Table IV., where the extrusions of these media are given as they appear after the indices have been corrected by the exponential law.

The removal of the irregularity in the extrusions, by the operation of this law, is particularly noticeable in the case of oil of anise; for there is thus made to disappear from this medium the apparent anomaly of its having its extrusions regular at temp. 13°·25 and temp. 20°·9, but irregular at temp. 15°·1, they being thus rendered regular at all the three temperatures.

It is interesting to note in this particular case the effect of the exponential law on the values of ϵ, the index of elasticity, as viewed in relation to the temperature.   These values become—

		Diff.
Temp. 13°·25 ϵ = 1·478427	. .	1044
,, 15°·1 1·477383	. .	3898
,, 20°·9 1·473485	. .	4942

These differences are not far from being proportional to the differences of temperature, but they may be brought into that precise ratio by a further slight alteration on the indices of refraction, while these latter may be at the same time preserved obedient to the exponential law.   This end may be attained by making the values of log ϵ_n and a_n stand thus :—

Temp. 13°·25 log ϵ_n 0·1857298 a_n 0·006160,

Temp. 15°·1   log ϵ_n 0·1857412 a_n 0·006303,

and   Temp. 20°·9   log ϵ_n 0·1847219 a_n 0·006350.

The values of ϵ then become

		Diff.
Temp. 13°·25 ϵ 1·478482	. .	1181
,, 15°·1 1·477301	. .	3733
,, 20°·9 1·473568	. .	4914

These differences are in exact proportion to the differences of temperature, while the values of ϵ themselves are in the inverse order of the temperature, thus strictly fulfilling the law.

The indices of refraction thus corrected will be found at the

end of Table VI., and may be regarded as more accurate than those calculated from the exponential law alone. It thus appears that when observations are made at more than two different temperatures and with sufficient accuracy, they furnish data whence the indices of refraction may be calculated so as to fulfil all those three laws, namely, the exponential law, the law of regular extrusions, and the law of temperature, as affecting the index of elasticity. When the indices of refraction fulfil these three conditions, they may be regarded as nearly quite correct.

In reference to the law of temperature, it will of course be understood that it is the differences of expansion under the influence of temperature, rather than the differences of the degrees of temperature themselves, to which the differences in the value of the index of elasticity must correspond.

To the effects of the exponential law, in bringing the extrusions into conformity with the regular type, a solitary exception is presented in the case of the oil of cassia. The singleness of this exception, however, raises a strong presumption that all media whatever conform to the same regular type as respects their extrusions, and that the apparent departure in the case of the oil of cassia is due solely to errors of observation. This is rendered the more probable by the fact of the proved existence of large errors in the observed indices of this medium; by the anomaly that, at the intermediate temperature, it has a higher exponent than at the higher and lower temperatures; and by the circumstance that the corrections deducible from the exponential law tend greatly to reduce, though not quite to remove, the irregularity in the extrusion of G, in which the departure of this medium from the regular type consists.

It would not be difficult to find for the oil of cassia a set of indices of refraction which, while fulfilling the exponential law, should at the same time render the extrusions regular, and also fulfil the law of temperature as respects the index of elasticity, taking advantage for this purpose of the analogies furnished by the oil of anise*. Owing, however, to the inherent inaccuracy

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* There seems to be some intimate connexion between the value of n, the exponent of least extrusion, and the position assumed by the nodes of the extrusions with the first powers of the normals. Thus, in the case of the oil of cassia, when the value of n is 3·4 or 3·5, the position of the lower node is considerably on the H side of G. But if the exponent be gradually lowered, the node will remove further and further from H, until, when the value of n is 2·9, the node nearly coincides with G itself. But when the exponent is still further reduced, the position of the node gradually advances towards F, and the extrusions then present the regular type. This is a point which invites further investigation; but for this purpose it would be needful to have a more accurate set of observations on oil of cassia.

There seems to be also some probability that the position of the upper

of the observations on oil of cassia, those indices could be regarded only as approximations to the truth; and such a result would hardly justify detention from a more important branch of this inquiry, namely, the institution of a comparison between the laws brought to light by the foregoing investigation, and the well-known hypothesis of M. Cauchy.

Suffice it meanwhile to have shown the high probability that in every case the extrusions and the laws governing them are of one uniform character,—a circumstance adding greatly to their interest and their importance as one of the means available for checking the accuracy of the indices, seeing that any departure from this normal type may be regarded as a very strong presumption of inaccuracy in the indices.

It remains, then, to compare the results obtained from the exponential law of the indices with those deducible from the hypothesis of M. Cauchy, "that the differences between the refractive indices of the medium are to each other very nearly as the differences between the reciprocals of the squares of the normal wave-lengths; or the refractive indices are each composed of two terms, whereof one is constant for the medium and temperature, the other reciprocally proportional to the squares of the normal wave-lengths."

It is on the basis of this law that the indices in Powell's Tables have been calculated. Those of the three lines B, F, and H are assumed from observation; and those of the four lines C, D, E, and G are thence calculated by means of formulæ based on the above law.

In order to exhibit more perfectly the differences between the results thus obtained and those derived from the law of a variable exponent, whose value depends on the proportion which the irrationality bears to the dispersion, the indices, as calculated from the law of M. Cauchy, are given in Table IX., being extracted from Powell's Tables; and the sums of the errors are compared with those arising under the exponential law.

Fully to appreciate the superiority of the latter, it is well to select a case in which the law of M. Cauchy wholly breaks down. For this purpose the highly dispersive medium, bisulphuret of carbon, will suffice. In this medium, the discrepancies arising under the law of M. Cauchy, between the observed and the calculated indices for the four lines C, D, E, and G, are

μC $-0\cdot000800$, μD $-0\cdot001700$, μE $-0\cdot002000$, μG $0\cdot004400$,   S $+0\cdot008900$.

The discrepancies arising on the *seven* lines under the exponen-

node between C and D may be found to coincide with that of the mean wave M, whose refractive index is that for white light; but this, too, is a point requiring further research.

tial law, the exponent for this medium being 2·5, are

μB+0·000143, μC−0·000252, μD+0·000047, μE−0·000110, μF−0·000295,

μG−0·000439, μH+0·000390, S±0·001675.

So that the sum total of these latter is less than the amount of the single error in D in the former case, and less than a fifth of the total errors arising under the law of M. Cauchy; consequently, in this important medium, the ratio in favour of the exponential law, as compared with that of M. Cauchy, is more than 5 to 1. The difference is still more striking if the individual discrepancies be compared,—the highest arising under the exponential law being only a tenth of that arising under Cauchy's law, the latter discrepancy, moreover, being far too large to be attributed to errors of observation; while those arising under the exponential law are all of such moderate magnitude, that there can be no hesitation in ascribing them to that cause.

From an inspection of Table IX. it will be seen that, as respects Fraunhofer's observations, the agreement between the calculated and observed indices is as 2 to 1 in favour of the exponential law*. In Rudberg's observations the ratio is as 4 to 3, and in Powell's as 10 to 7, while from the three sets combined it is as 6 to 4. But the best criterion of judgment is furnished by those media which have a high dispersive and extrusive power, and in which the law of M. Cauchy entirely fails, presenting discrepancies far too great to be attributed to experimental error. Such are those in the case of the bisulphuret of carbon above noted; such are also the large discrepancies in the case of the oil of cassia, ranging between 0·0017 and 0·0029, while the largest individual discrepancy arising under the exponential law is under 0·001. In some few instances it will be observed that the result appears to be in favour of the law of M. Cauchy, but these anomalies are all clearly traceable to experimental error. Looking at the results as a whole, there can be no doubt that the decided superiority rests with the exponential law, as being the true law of the indices.

The great defect in the hypothesis of M. Cauchy is its failure to accommodate itself to the phænomenon of irrationality and the attendant extrusion of the fixed lines. Its apparent agreement with observation in a considerable number of cases, arises simply from the circumstance that, with the squares of the normals, the extrusions are in those cases so small that they may be eliminated without *greatly* affecting the indices; and it is only when

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* It must be kept in view, in examining this Table, that the normals on which Powell's calculations are based differ slightly from those specified in this paper; but this circumstance does not materially affect the general results.

the dispersive and extrusive powers of the medium are large that its inapplicability becomes manifest.

The exponential law, on the other hand, entirely overcomes the difficulty arising out of the irrationality; because it shows that in each medium there is, dependent on the proportion which the irrationality bears to the dispersive power, a certain exponent for the normals at which the extrusions attending the irrationality are reduced to a minimum, and that with this exponent the indices may always be obtained from two constants,—each index being then reduced to two terms, one of which, ϵ_n, is constant for the medium temperature and exponent; while the other (a_n) corresponds to a further shortening of the wave-length within the medium, which is constant for each wave, and so inversely proportional to the primary wave-lengths of the normals, with this particular exponent applied to them, the formula for each

index being $\mu = \dfrac{\lambda^n}{\dfrac{\lambda^n}{\epsilon_n} - a_n}$ *.

[To be continued.]

XXXV. *Remarks on* Mr. Harley's *paper on Quintics.* By G. B. Jerrard†.

IN the 'Quarterly Journal of Pure and Applied Mathematics' for last January, there is a paper by Mr. Harley "On the Theory of Quintics," respecting which I am induced to offer a few remarks.

1. On comparing the results at which he has arrived,

$$t^6 + 5\mathrm{Q}\mathrm{E}t^2 + \sqrt{\{\mathrm{E}(\mathrm{E}^3 - 108\mathrm{Q}^5)\}} . t - 5\mathrm{Q}^4 = 0, \quad . \quad (\omega_1)$$

$$t_1 t_3 + t_2 t_5 + t_4 t_6 = x_1(3^2\mathrm{Q} - 2x_1^3), \quad . \quad . \quad . \quad . \quad (\omega_2)$$

in his explanation of Mr. Cockle's 'Method of Symmetric Products,' we may easily perceive (for Q, E are the coefficients of the trinomial equation in x with which he sets out) that the method in question is in general not applicable to equations of the fifth degree.

For as the equation (ω_1) belongs, according to art. 8 of Mr. Harley's paper, to a class of equations of the sixth degree, solved by Abel, the roots of which, as is well known, do not involve any radical higher than a cubic, it is manifest, from (ω_2), that

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* The refractive index of any medium at a given temperature, for *white* light, may be found very accurately from the above formula by making $\lambda = 0.933494$, the length of the mean wave, in relation to that corresponding to the fixed line B as unity.

† Communicated by the Author.

the roots of the trinomial equation also must, if there be no error in the processes, admit of being expressed by means of radicals characterized by the symbols $\sqrt[2]{}$, $\sqrt[3]{}$ only, that is to say, without the aid of any function of the form $\sqrt[5]{z}$. And in effecting the reduction of the general equation to the trinomial one, no such function is introduced.

The method, therefore, even should it be found to extend beyond the extreme case of $x_1=0$, $x_2=0$, .. $x_5=0$, cannot be applied except when $\sqrt[5]{z}$ (I mean an irreducible radical of that form) does not enter into the expressions for the roots.

2. *Postscript.*—Since writing these lines, I have seen a paper by Mr. Cockle in the Philosophical Magazine for the present month. The objection which attaches to his method from the absence of quintic surds is not overlooked. But, instead of confining himself to the elucidation of the origin of an error, the existence of which is so obvious, he goes on to infer, from the failure of his method, the impossibility of effecting, except in a limited number of cases, the algebraical resolution of equations of the fifth degree,—a result in the accuracy of which I cannot concur. It is true that all methods of solution, if equally general, must ultimately coincide. The success of one involves the success of all*. The failure of one the failure of all. It is, however, far otherwise with respect to methods subject to conditions not necessarily, or rather not universally, inherent in the subject.

3. But I come to what Mr. Cockle brings forward as his weightiest objection to my method. He says, "The error of Mr. Jerrard inheres, in my opinion, in his mode of comparing the equations (ab) and (ac) at pages 80 and 81 of his most valuable 'Essay.' His functions $_1\Xi$, $_2\Xi$, $_3\Xi$, and $_4\Xi$ in art. 104 are foreign to the question, mere instruments for eliminating radicalities. They lead to no other result than that to which the immediate comparison of (ac) and

$$\Xi - {}_0\Xi = 0$$

would conduct us, viz. an expression for Ξ into which $P_{f(\beta,\epsilon)}$ enters irrationally." Doubtless the result in both cases would be the same. On substituting the expression for $_0\Xi$ in (ac) we should find by the evanescence of N_3, N_2, N_1, N_0 that

$$N_{4, f(\beta,\epsilon)} = {}_0\Xi_{f(\beta,\epsilon)} = \Xi_f,$$

* See applications II. III. at pp. 84, 85 of my 'Essay on the Resolution of Equations,' published by Taylor and Francis, Red Lion Court, Fleet Street, London.

just as before. The irrationality is merely one of form*. What then? In the place of an objection to my method, has sprung up a verification of it.

All Mr. Cockle's other objections tend, in like manner, by their failure, only to make more palpable the validity of the method to which they are opposed. But of these hereafter.

March 1860.

XXXVI. *Is the Problem, " How far is the mass of the earth solid and how far fluid?" excluded from the domain of positive Science? By the* Venerable JOHN HENRY PRATT, *Archdeacon of Calcutta.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE question I have placed at the head of this paper, Prof. Haughton has answered in the affirmative in his paper in the Transactions of the Royal Irish Academy, vol. xxii. p. 251, " On the Original and Actual Fluidity of the Earth and Planets." If this conclusion be correct, it must render altogether useless such investigations as that by Mr. Hopkins regarding the thickness of the earth's crust. This consideration invests Professor Haughton's conclusion with so much importance, that it demands attentive examination. It was upon this ground that I pointed out in your Number for May 1859, what I conceived to be—and still conceive to be—a fallacy in the reasoning which brought out this conclusion. In his last paper, in your Number for December, which reached me yesterday, Professor Haughton does not, in my view, clear away the difficulty.

2. In his original communication to the Irish Academy he deduces the following equation (I here use his own notation) :—

$$\frac{e}{a}\int_0^a \rho a^2 - \frac{1}{5a^3}\int_0^a \rho \frac{d \cdot a^5 e}{da} - \frac{a^2}{5}\int_a^a \rho \frac{de}{da} - \frac{ma^2}{2a^3}\int_0^a \rho a^2 = 0, . (12)$$

and by differentiation obtains from it

$$\frac{d^2 e}{da^2} + \frac{2\rho a^2}{\int_0^a \rho a^2} \frac{de}{da} - \frac{6e}{a^2}\left(1 - \frac{\rho a^3}{3\int_0^a \rho a^2}\right) = 0. \quad . \quad . \quad . \quad (13)$$

Two lines further on he states that this is "independent of the law of density and ellipticity of the solid parts of the earth."

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* At the time of writing art. 104 of my ' Essay,' this second mode of arriving at the equation

$$\Xi_f - \mathfrak{r}\{P_{f(\beta, \epsilon)}\} = 0$$

presented itself to my mind. But I did not like to deviate too widely from the route I had taken in 1845.

It is this last statement which I controvert. In my paper in your Number for May last (page 329, line 23), I assert that equation (13) "assumes that the law of density and ellipticity is continuous throughout the whole mass, solid and fluid, the solid parts lying in strata of the form and density they would have if they were wholly fluid."

3. Professor Haughton, in your Number now received, replies to my reasoning by showing that he has *differentiated* equation (12) right. This I never called in question. "The question at issue" between us is not, as his "mathematical friend" states, "to determine a rule for differentiating this equation [viz. (12)] with regard to *a*." What I assert is, that certain terms of the differentiated equation will not cancel each other so as to produce equation (13), *unless* we make such an assumption as involves this principle,—That the same law of density and ellipticity belongs to the solid and fluid parts. This I will now show more fully.

4. I would first, however, observe that equations (12), (13) apply equally to the solid and fluid portions of the mass. Equation (12) expresses the law, that the resultant of all the forces acting on any particle is at right angles to the layer or surface in which the particle lies. This law is essential to the equilibrium of the fluid part. It is also tacitly taken to be true for the solid parts by Professor Haughton. For he differentiates equation (12) with respect to *a*, and therefore assumes that equation (12) holds at the immediate neighbourhood, *on both sides,* of the surface to which *a* belongs. In the case in which this surface is the bounding surface between the solid and fluid parts, the mass is solid on one side and fluid on the other. Hence equation (12) applies to both the solid and fluid portions.

5. To banish the integrals from equation (12) and obtain equation (13), we must multiply by a^3, differentiate with respect to *a*, divide by a^4, and differentiate again. The result is equation (13). The first differentiation produces, from the second term, a term $-\frac{1}{5}\rho a^5 \frac{de}{da}$; and, from the third term, a term $+\frac{1}{5}\rho a^5 \frac{de}{da}$. It is assumed that these terms cancel each other. So also the second differentiation produces, from the second term, a term $+\rho \frac{de}{da}$; and, from the third term, a term $+\rho \frac{de}{da}$. These are assumed to be equal to each other; that is, $\rho \frac{de}{da}$ is assumed to have the same value on *both sides* of the surface of which *a* is the mean radius. This will be the case when this surface is one of the layers wholly within the fluid, or wholly within the solid part, even though the laws of density and ellipticity of the fluid

and solid parts are different. But it will not be the case at the *bounding surface* between the solid and fluid parts, *unless* the values of $\rho \dfrac{de}{da}$, derived from the law of the fluid and from the law of the solid layers, are the same at this bounding surface. As ρ is some function of a, e is some function of a (as equation (13) shows); hence $\rho \dfrac{de}{da}$ is some function of ρ only. Hence, then, the laws of density of the solid and fluid layers may be different, as far as the reasoning at present has carried us; but the two laws must give the same amount of density at the bounding surface, otherwise equation (13) does not follow from equation (12).

6. Thus far, then, this is the result we are come to. The mass consists of solid layers following a certain law of density, and of fluid layers following the same or another law of density; but at the surface where they meet the density is the same. Also the resultant force acting at any point of any layer, solid or fluid, is at right angles to that layer.

7. Now the equilibrium of this mass will not be disturbed if the first solid layer, reckoning from the inner surface towards the outer one, become fluid, retaining its density. For the density of that one layer accords with the fluid law, and the forces acting on the layer are perpendicular to its surface.

Equation (13), therefore, holds for the bounding surface between the first and second solid layers only on the same terms that it holds for the bounding surface between the solid and fluid portions. From this it follows that the density of the second solid layer must follow the fluid law, and so in succession with all the rest; and therefore the law of density of the solid and fluid portions must be the same throughout the whole mass, if equation (13) is a correct inference from equation (12).

8. This result appears to me to be *à priori* evident without this proof. For Professor Haughton will acknowledge that the equilibrium of the fluid parts will hold, if the solid parts *do* follow the law of the fluid parts. But if any different distribution of the solid parts take place, their resultant attraction on the fluid cannot possibly be the same as before on every particle of the fluid. The conditions of equilibrium would therefore not hold, and the equilibrium would become impossible without a change.

9. With regard to the other subject touched upon in Professor Haughton's last paper—the argument drawn from the Himmalaya Mountains, and the Ocean south of India, in my paper in your Number for November—I will simply make the following observations.

(1) The mountain mass must not be considered as one rigid mass of rock without natural joints. The Himmalaya Mountains are far too irregular in their structure, and too full of gigantic cracks and joints in all directions to allow of our applying the principle of the arch in the way Professor Haughton suggests. Moreover, if the cross strain in his arch, of 500 miles span and thickness of only one quarter of a mile at the spring, is not sufficient to *compress* the materials of the rock, it will surely break off angles, as I have mentioned in art. 2, p. 346 of the paper alluded to, and a catastrophe would ensue.

(2) If Professor Haughton will not admit this, and still thinks that the principles of the arch should be applied to the crust under the Himmalayas, what will he say to the second part of my paper, in which the upward effect of the ocean is considered? Here the arch cannot possibly act.

(3) Professor Haughton notices a mistake I made in omitting a 2 in my calculation; but he observes that, as it does not seriously affect my result, he lays no stress upon it. This mistake (which also occurs, I fear, in a treatise on "Attraction, Laplace's Coefficients, and the Figure of the Earth," which by this time is, I suppose, published) I detected about a month ago, when it was too late to correct it. The calculation, however, in which it occurs is not to find the actual thickness of the crust, as will be obvious to my readers, but only to show that it is very thick. Where the mistake occurs in your Journal, the result I bring out is that the thickness of the crust in the middle of the mass and at the end is 581 and 576 miles; whereas if the mistake had not occurred, it would have come out 581 and 570 miles, which not only does not affect my conclusion regarding the great thickness seriously, but in fact not at all.

Calcutta, January 21, 1860.

XXXVII. *Chemical Notices from Foreign Journals.* By E. ATKIN-SON, *Ph.D., F.C.S., Teacher of Physical Science in Cheltenham College.*

[Continued from p. 216.]

ESPENSCHIED*, in a recent dissertation, has described a compound of nitrogen and selenium obtained by the action of ammoniacal gas on sublimed chloride of selenium, $SeCl^2$. The action is so very violent that the ammoniacal gas must be diluted with a large volume of hydrogen, and the vessel in which the action takes place carefully cooled. The chloride gradually becomes green, and ultimately changes into a brown mass, in-

creasing considerably in bulk. This mass is placed in water, wherein it separates as a brick-red powder, which is collected on a filter and dried over sulphuric acid.

By a blow or by friction, or by being heated, this substance explodes with a loud report, forming clouds of selenium vapour. It is not pure nitride of selenium, but contains an admixture of selenium. This latter can be dissolved out by digestion with solution of cyanide of potassium, which leaves the nitride of selenium unattacked.

Pure nitride of selenium is an orange-yellow powder which undergoes no change at 150°, but explodes at 200°. It is extremely explosive; the smallest pressure causes a detonation. It also explodes when placed in chlorine, or in hydrochloric acid. It is dissolved by nitric acid with formation of ammonia and selenious acid. By treatment with potass, ammonia is liberated, and selenate and selenide of potassium formed. Hypochlorite of soda converts it into selenate of soda, with liberation of hydrogen. Heated with water in a sealed tube to 150°—160°, it is completely changed into ammonia, selenious acid, and pure selenium.

Espenschied's analyses did not give very concordant results. The mean numbers obtained were, selenium 83·69, nitrogen 16·33, which corresponds pretty closely to $Se^2 N$. The body, however, most likely contains hydrogen, and the formula

$$Se^6 N^3 H = Se^2 NH + 2 Se^2 N,$$

which Espenschied considers probable, requires selenium, 84·57; nitrogen, 15·07; hydrogen, 0·36.

Experiments by Espenschied to obtain a corresponding tellurium compound have not given the expected results. Chloride of tellurium, $TeCl^2$, absorbs ammonia, forming a greenish-yellow mass which is not deliquescent. It consists of $TeCl^2 + 2 NH^3$ [*].

Ufer has investigated nitride of chromium [†]. In preparing it he tried several methods, but found none superior to Schrötter's, which consists in passing ammoniacal gas over sesquichloride of chromium heated to a high temperature. The violet chloride gradually changes, becomes first dark green, and ultimately black. Vapours of chloride of ammonium are given off, and when these cease to appear, the reaction is complete. At the close of the experiment, as high a heat is required as the tube will bear. The decomposition is thus expressed:—

$$Cr^2 Cl^3 + 4 NH^3 = Cr^2 N + 3 NH^4 Cl.$$

Sesquichloride   Nitride
of chromium.   of chromium.

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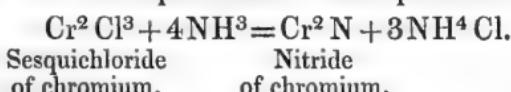
[*] Which might be written $NH^4 Cl$, $NH^2 TeCl$.
[†] Liebig's *Annalen*, December 1859.

Nitride of chromium contains some undecomposed sesquichloride which cannot be removed completely by its being heated in ammonia. Ufer purified it by an application of an observation of Peligot. That chemist found that a very small quantity of protochloride of chromium can convert a large quantity of the insoluble violet chloride into the soluble modification. Accordingly by digesting the impure nitride with zinc and hydrochloric acid, the nascent hydrogen (which does not attack the nitride) reduces a portion of the sesquichloride to protochloride, which then renders the remainder soluble; by washing the residue with water, any sesquichloride is completely removed.

The best method of determining the chromium is to convert the nitride into sesquioxide by heating it in oxygen. Ufer also determined the chromium by fusing the nitride with carbonate of soda and nitre, and estimating the chromic acid thus formed by obvious methods. The analyses gave as a mean result 79·1 per cent. chromium, and 20·9 per cent. nitrogen; agreeing well with the formula NCr^2, which requires 79·2 chromium, and 20·8 nitrogen. Schrötter assigned to this substance the formula N^2Cr^5. But it is probable that Schrötter's preparation contained some undecomposed chloride. Ufer's formula for nitride of chromium is rendered probable, not only by his analysis, but by the mode of its formation, which is further quite analogous to the formation of nitride of boron from boracic acid and ammonia.

$$BO^3 + NH^3 = 3HO + BN.$$

Nitride of chromium is a black amorphous powder. It has the remarkable property (which it shares with analogous compounds of tungsten) of decomposing ammoniacal gas into its constituents when passed over it at a high temperature; and in its preparation, towards the close of the operation, there is a moment in which the smell of ammoniacal gas is no longer perceived, but instead of it nitrogen and hydrogen gases appear.

Nitride of chromium is a very permanent body. It is with difficulty attacked by the strongest acids. Heated in the air, it is converted into oxide of chromium. It is not attacked by fusing caustic potash, nor by fused carbonate of soda; but heated with aqueous potash in a sealed tube to 190° it is converted into chromate, and when fused with nitre a deflagration takes place. It is dissolved by alkaline hypochlorites with disengagement of nitrogen gas.

When gently heated in a current of chlorine, small explosions at first take place, probably arising from the formation of some

chloride of nitrogen; the greater part of the mass is converted into violet sesquichloride of chromium, which volatilizes.

$$Cr^2N + 6Cl = Cr^2Cl^3 + NCl^3.$$

| Nitride of | Sesquichloride | Chloride of |
| chromium. | of chromium. | nitrogen. |
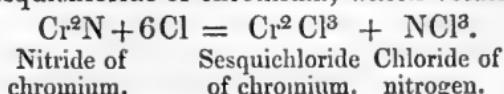
By dry hydrochloric acid gas it is with difficulty decomposed at a high temperature into sal-ammoniac and sesquichloride of chromium,

$$Cr^2N + 4HCl = NH^4Cl + Cr^2Cl^3.$$

When nitride of chromium, placed in a covered crucible and covered with a layer of borax, is heated in a blast furnace, it is converted into metallic chromium, a small residue of nitride of chromium being left.

M. Degousse, a goldbeater in Paris, has succeeded[*] in preparing aluminium in fine plates like gold or silver. The operation of beating is effected in the usual manner, but it is necessary that the reheating be more frequent; the fire of a chauffer is most suitable. Aluminium-leaf may replace silver in many cases; its white, though less brilliant, is more durable.

Wöhler[†] has the following remarks on Degousse's leaf-aluminium. It is readily combustible; if held in the edge of a spirit-lamp flame, it takes fire and burns with great brilliancy. It is very thin; a cubic inch only weighs a milligramme. If a leaf of it be pressed together, placed in a bulb and heated by means of a spirit-lamp in a current of oxygen, it burns instantaneously with a dazzling lightning-like appearance. The resultant alumina is fused, and as hard as corundum. Aluminium wire also burns in oxygen like iron; but the combustion does not proceed far, for the next parts melt away before they have reached the temperature of combustion. Aluminium in the compact form does not decompose water, but the leaf, when placed in boiling water, decomposes a sufficient quantity to enable the hydrogen to be collected. The metal assumes at first a faint bronze surface colour. After several hours' boiling, the laminæ become partially translucent, that is, converted into alumina. If the residue be treated with hydrochloric acid, the unoxidized metal is dissolved, while the alumina remains undissolved.

Several of the noble metals, but more especially platinum in the finely divided state, have the property of causing a disengagement of oxygen when placed in solution of peroxide of hydrogen. For this enigmatical phænomenon Schönbein[‡] suggests an explanation based on the following reactions.

* Barreswill's *Répertoire de Chimie*, October 1859.
† Liebig's *Annalen*, February 1860.
‡ Poggendorff's *Annalen*, January 1860.

1. Guaiacum resin solution gives with free as well as combined ozonized oxygen a very delicate blue colour, while the active oxygen of peroxide of hydrogen and of the antozonides are without action upon it. But when platinum-black is added to a solution of guaiacum which contains peroxide of hydrogen, an intense blue colour is formed.

2. Ether dissolves peroxide of hydrogen without being affected by it, while free or combined ozonized oxygen at once renders it acid. Now if an ethereal solution of peroxide, and which at once gives an intense blue with chromic acid, be agitated with platinum-black, it loses the property of giving a blue with chromic acid, and soon has an acid reaction.

3. Peroxide of hydrogen decolorizes indigo solution very slowly, while it is instantaneously decolorized by free or combined ozonized oxygen. If, however, a mixture of indigo solution and peroxide of hydrogen be agitated with platinum-black, the solution is rapidly decolorized.

Hence it appears that the Θ of the peroxide produces the same actions as the Θ of ozonized oxygen. May it not then be assumed that platinum can change the positively active oxygen of the peroxide into the negatively active state, without making any assumption as to how this is effected. On this assumption the Θ of the peroxide in immediate contact with the platinum becomes changed into negatively active oxygen, Θ; this Θ would neutralize a portion of $\oplus$ and form ordinary oxygen; in other words, the layers of peroxide next the platinum would form ordinary oxygen and water. After this catalysis, the $\oplus$ of another portion of peroxide in contact with the platinum would be changed into Θ, which would decompose with another portion of peroxide of hydrogen into water and oxygen, and so on. A small quantity of platinum might thus decompose an indefinite quantity of peroxide.

To saturate ether with peroxide of ethyle, Schönbein uses the following method:—Dilute hydrochloric acid is added to a gramme of peroxide of barium until the liquid is neutral; the mixture is then agitated with 40 grms. of pure ether and allowed to stand. This ethereal solution, poured off from the chloride of barium, turns chromic acid blue, decolorizes permanganate of potash, and has indeed all the reactions of peroxide. It may be distilled without alteration. When a volume of it is shaken with four volumes of water, the peroxide of hydrogen is completely removed. Potash removes it still more quickly.

Nièpce de Saint-Victor and Corvisart* describe the following instances of the peculiar influence which the sunlight exerts in modifying and changing amylaceous substances.

* *Comptes Rendus,* September 5, 1859.

If two 1 per cent. solutions of starch be prepared under the same circumstances, and if one of them be kept in the dark and the other exposed to the sunlight, the latter will be found to exert an action on the polarizing apparatus ; more dextrine and sugar have been formed. If very weak solutions be taken (about $\frac{1}{2000}$) and exposed to the sunlight for about eighteen hours, it will be found that the solution has lost the properties of the original amylum, and more resembles inuline.

Many substances, such as lactate or citrate of iron, and corrosive sublimate, limit or neutralize this action of the light; while other substances, such as potassio-tartrate of iron, or nitrate of uranium, greatly increase it.

Dextrine and cane-sugar are unaffected by light.

There is a curious action on oxalic acid. If a 4 per cent. solution of the acid be mixed with a 1 per cent. solution of nitrate of uranium, and the mixture boiled for even a considerable length of time, *provided this is done in the dark,* no change takes place. But if the light, even of a clouded sky, have but a momentary action, a decomposition, evidenced by the disengagement of gas, at once sets in ; and if the mixture be placed in the sun, a quantity of carbonic oxide may be collected. That this action is due neither to the temperature nor to the free acid, is evident from the fact that at a temperature of zero, and with the employment of oxide of uranium, the same results are obtained.

Direct experiments have shown that animal starch (glycogenous substances) is more rapidly changed into sugar in the light than in the dark ; and, remarkably enough, nitrate of uranium decreases instead of increases the action.

It is remarkable that animal starch in frogs' liver is not changed into sugar in winter, which is also the case with the vegetable starch.

This might explain why the sugar-forming substances which are so abundant in the membrane of the fœtus immediately disappear after birth.

It can scarcely be doubted that light plays a slow but very powerful part in effecting changes in the animal body ; and it is evident that a knowledge of the substances which accelerate or lessen this action is of great importance in medicine. Th symptoms of diabetes, and the action which light has been observed to exert on scrofulous persons, may be adduced as cases in point.

M. Cloez has described* two new benzoic compounds. When cyanate of potash is mixed with chloride of benzoyle, and the mixture heated to nearly the fusing-point of the cyanate in an

* *Répertoire de Chimie,* January 1860.

assay flask, carbonic acid is given off, and chloride of potassium formed, with which remains associated a new body, *cyaphenine*. Some benzonitryle is formed at the same time. Cyaphenine is volatile without decomposition, and may therefore be separated from the chloride by heat; or the chloride may be dissolved out by water.

Cyaphenine has the formula $C^{42} H^{15} N^3$; it corresponds to cyanethine, $C^{18} H^{15} N^3$, discovered by Frankland and Kolbe. The latter body may be regarded as a triple molecule of cyanide of ethyle, $C^{18} H^{15} N^3 = 3(C^4 H^5 C^2 N)$; so cyanethine may be considered as a triple molecule of benzonitrile or cyanide of phenyle, $C^{42} H^{15} N^3 = 3(C^{12} H^5 C^2 N)$. The formation of cyaphenine may be thus expressed:—

$$3C^{14} H^5 O^2, Cl + 3KO, C^2 NO = C^{42} H^{15} N^3 + 3 KCl + 6 CO^2.$$
Chloride of   Cyanate of   Cyaphenine.
benzoyle.   potash.
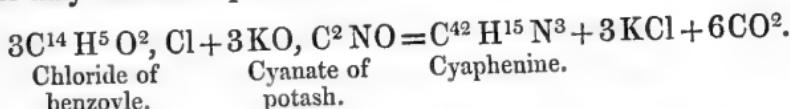
Cyaphenine is a neutral, solid, hard substance, with a crystalline fracture; it fuses at 224°, and distils at 350°. It is little soluble in absolute alcohol. Potash decomposes it, liberating ammonia. Treated by strong nitric acid it yields a crystallized nitro-compound, $C^{42} H^{12} (NO^4)^3 N^3$.

Thiobenzoic Acid, $C^{14} H^5 O^2 S$, HS.—This body corresponds to thioacetic acid, obtained by Kekulé by the action of pentasulphide of phosphorus on acetic acid. It is prepared by adding chloride of benzoyle to an alcoholic solution of hydrosulphide of potassium. It crystallizes from bisulphide of carbon in rhomboidal plates. When pure it is inodorous and tasteless. It fuses at 120°, and begins to decompose at about 160°—180°. It is not soluble in water, and but slightly so in alcohol and in ether. Its best solvents are sulphur compounds, such as mercaptan, sulphide of ethyle, and bisulphide of carbon. It combines with potash, soda, and ammonia to form definite crystallized salts, from which the acid is liberated unchanged on the addition of hydrochloric acid.

Kekulé's thiacetic acid has been obtained by Jacquemin and Vosselmann[*] by the gradual addition of chloride of acetyle to hydrosulphide of potassium.

Carius, in a paper[†] on the equivalent substitution of oxygen by sulphur, describes several new sulphur compounds.

The action of pentasulphide of phosphorus had been interpreted by Kekulé as being analogous to that of pentachloride of

[*] *Comptes Rendus*, vol. xlix. p. 371.
[†] Liebig's *Annalen*, November 1859.

phosphorus, and as taking place in the following manner :—

$$5(C^2 H^6 O) + P^2 S^5 = 5(C^2 H^6 S) + P^2 O^5.$$

Alcohol.　　Pentasulphide　　Mercaptan.　　Anhydrous
　　　　　of phosphorus.　　　　　　phosphoric acid.
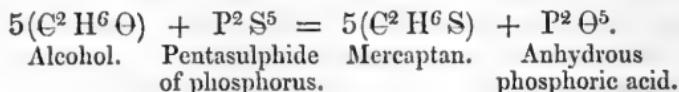
A careful study of the reaction has led Carius to a different con-
clusion. He finds that sulphuretted hydrogen is constantly dis-
engaged, and that mercaptan is only obtained when the mixture
becomes much heated. Carius compares the reaction to that of
anhydrous phosphoric acid on alcohol, and expresses it by the
equation

$$5(C^2 H^6 O) + P^2 S^5 = 2 H^2 S + \left.\begin{matrix} PS \\ (C^2 H^5)^2 H \end{matrix}\right\} O^3 + \left.\begin{matrix} PS \\ (C^2 H^5)^3 \end{matrix}\right\} O^2 S.$$

Alcohol.　　Pentasulphide　　　　　New body.　　　　New body.
　　　　of phosphorus.
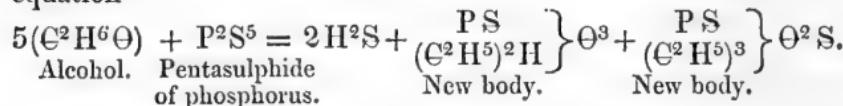
Both the new substances are formed in some quantity. The
first of them is an acid, which Carius names *diethylsulphophos-
phoric acid*, and is diethylphosphoric acid in which the oxygen
of the radical phosphoryle is replaced by sulphur. It is a vis-
cous oily liquid, with a very sour and bitter taste. It is a stable
body, but when heated it decomposes with formation of mercap-
tan. It forms a series of well-defined salts. The lead and zinc
salts have the peculiarity of being precipitated in oily drops, and
of solidifying when touched with a hard body. The salts, like
the acid, are inodorous.

The other body formed as above is *disulphophosphate
of ethyle*, $\left.\begin{matrix} PS''' \\ (C^2 H^5)^3 \end{matrix}\right\}\begin{matrix} O^2 \\ S \end{matrix}$. It is a colourless oily liquid, with an
aromatic but somewhat alliaceous odour. By treatment with
hydrosulphide of potassium it yields the potassium salt of a new
acid, *diethyldisulphophosphoric acid* and mercaptan. Thus,

$$\left.\begin{matrix} PS''' \\ (C^2 H^5)^3 \end{matrix}\right\}\begin{matrix} O^2 \\ S \end{matrix} + KHS = \left.\begin{matrix} PS''' \\ (C^2 H^5)^2 K \end{matrix}\right\}\begin{matrix} O^2 \\ S \end{matrix} + C^2 H^6 S.$$

Disulphophos-　　Hydrosulphide　　Diethyldisulphophos-　　Mercaptan.
phate of ethyle.　of potassium.　　phate of potassium.
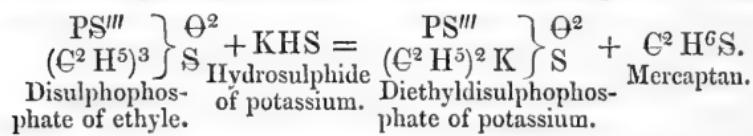
The action of mercaptan on disulphophosphate of ethyle is very
interesting as leading to the formation of mixed sulphur-ethers.
The formation of a mixed sulphur-ether of ethyle and methyle
takes place as follows :—

$$\left.\begin{matrix} PS''' \\ (C^2 H^5)^3 \end{matrix}\right\}\begin{matrix} O^2 \\ S \end{matrix} + C H^4 S = \left.\begin{matrix} PS''' \\ (C^2 H^5)^2 H \end{matrix}\right\}\begin{matrix} O^2 \\ S \end{matrix} + \left.\begin{matrix} C\ H^3 \\ C^2 H^5 \end{matrix}\right\} S.$$

Disulphophos-　　Methyle-　　Diethyldisulpho-　　New sulphur-
phate of ethyle.　mercaptan.　phosphoric acid.　　ether.
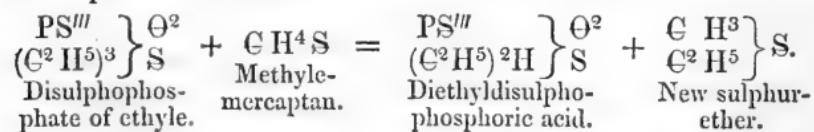
Carius has prepared the corresponding mixed sulphur-ether,
$\left.\begin{matrix} C^5 H^{11} \\ C^2 H^5 \end{matrix}\right\} S$, and will shortly describe it.

By the action of pentasulphide of phosphorus on mercaptan, or better on mercaptide of mercury, Carius has obtained *tetrasulphophosphate of ethyle*, or phosphate of ethyle in which all the oxygen is replaced by sulphur.

$$5(C^2H^5HgS) + P^2S^5 = 2Hg^2S + \begin{matrix}PS'''\\(C^2H^5)^2Hg\end{matrix}\Big\}S^3 + \begin{matrix}PS'''\\(C^2H^5)^3\end{matrix}\Big\}S^3.$$
<p style="text-align:center">Mercaptide of mercury.   Tetrasulphophosphate of ethyle.</p>
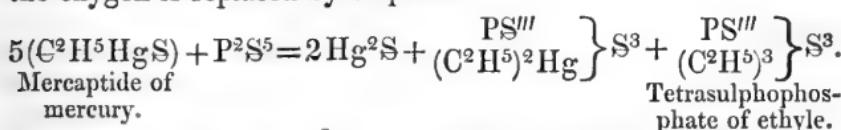
Tetrasulphophosphate of ethyle is an oily, clear, yellow liquid, which is very like disulphophosphate of ethyle, but more decomposable.

Alloxan, $C^8H^2N^2O^8$, and parabanic acid, $C^6H^2N^2O^6$, both products of the oxidation of uric acid, exhibit a great analogy of properties; both are decomposed by alkalies in a similar manner, and yield homologous products. By the action of reducing bodies on alloxan, a substance, alloxantine, is formed. Limpricht has found[*] that, by the action of nascent hydrogen on parabanic acid, a corresponding compound, *oxalantine*, is formed:—

$$2(C^6H^2N^2O^6) + 2H = C^{12}H^4N^4O^{10} + 2HO$$
<p style="text-align:center">Parabanic acid.   Oxalantine.</p>
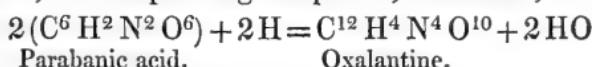
It is prepared by adding dilute hydrochloric acid and zinc to a solution of parabanic acid; a slow disengagement of hydrogen takes place, and a white crystalline compound of oxalantine and zinc is formed. When this is treated with sulphuretted hydrogen, and the solution evaporated, oxalantine is obtained in crystalline crusts. It is little soluble in water, and almost insoluble in alcohol and ether.

Alloxan, by the action of different cyanides, undergoes different decompositions. When treated with cyanide of potassium, dialurate of potash is formed; but if cyanide of ammonium be used, the result, as Rosing and Schischkoff have shown[†], is different. They found that a new body, *oxalan*, to which they ascribed the formula $C^{30}H^{26}N^{14}O^{30}$, was formed in the reaction. Liebig, who had previously observed this reaction, examined the body[‡], and found that the formation of oxalan might serve as a test for the presence of alloxan in animal fluids. If to a liquid containing only a small quantity of alloxan, hydrocyanic acid and then ammonia be added, a precipitate of oxalan is formed. Although he did not propose a new formula for the body, he analysed it, and pointed out that the ratio of the carbon to the nitrogen was as $2:1\S$.

Strecker has subsequently examined§ the formation of oxalan,

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* Liebig's *Annalen*, August 1859.   † Ibid. vol. cvi. p. 255.
‡ Ibid. vol. cviii. p. 126.   § Ibid. January 1860.

and confirms the previous statements. In its formation the proportion of hydrocyanic acid is immaterial; a very small quantity can convert a large quantity of alloxan into oxalan. It takes no part in the reaction, and merely serves as a sort of ferment. It is known that hydrocyanic acid effects in a similar manner the conversion of hydride of benzoyle into the polymeric benzoin. When a tolerably concentrated solution of alloxan is used, dialurate of ammonia is formed. Strecker's analyses give for oxalan the formula $C^6 H^5 N^3 O^6$; and he expresses its formation thus:

Alloxan	.	$C^8 H^2 N^5 O^8$		C^6	$H^5 N^3 O^6$	Oxalan.
Alloxan	.	$C^8 H^2 N^2 O^8$		$= C^8$	$H^4 N^2 O^8$	Dialuric acid.
Water	. .	$H^2 \quad O^2$		C^2	O^4	Carbonic acid.
Ammonia	.	$H^3 N$				

$$C^{16} H^9 N^5 O^{18} \qquad C^{16} H^9 N^5 O^{18}$$
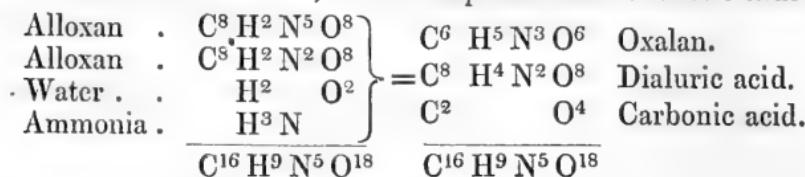
It may be regarded as the amide of oxaluric acid, *oxaluramide.* By treatment with water it is resolved into oxalurate of ammonia, a part of which is further decomposed into oxalic acid and urea.

$$C^6 H^5 N^3 O^6 + 2 HO = NH^4 O, C^6 H^3 N^2 O^7.$$

$$\text{Oxalan.} \qquad\qquad \text{Oxalurate of ammonia.}$$
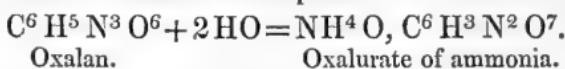
Compounds analogous to oxalan, but containing ethyle, methyle, or phenyle in the place of hydrogen, may be obtained by substituting in the preparation the corresponding amide base for ammonia.

If to a solution of alloxan containing hydrocyanic acid, carbonate of potash is added to alkaline reaction, carbonic acid is disengaged and dialuric acid separates, and the solution contains oxalurate of potash. Thus,

$$2(C^8 H^2 N^2 O^8) + 2 HO + 2 KO = C^8 H^3 K N^2 O^8 + C^6 H^3 K N^2 O^8 + 2 CO^2$$

$$\text{Alloxan.} \qquad\qquad\qquad\qquad \text{Dialurate of} \quad \text{Oxalurate of}$$
$$\text{potash.} \qquad \text{potash.}$$
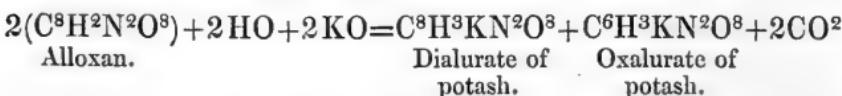
Both in the case of the decomposition of alloxan by cyanide of ammonium and by cyanide of potassium, the hydrogen of the water changes part of the alloxan into dialuric acid, while the oxygen oxidizes a like quantity into carbonic acid and parabanic acid. In the presence of ammonia the parabanic acid forms *oxaluramide,* in the presence of fixed alkalies an oxalurate.

Pure ferro- and ferri-cyanide of potassium have no action on alloxan solution; but if, as is frequently the case, they contain traces of cyanide of potassium, oxaluramide is formed. A solution of alloxan might accordingly be used to detect the presence of cyanide of potassium in ferro- or ferri-cyanide of potassium.

Strecker suggests, and will pursue the question, whether the poisonous action of hydrocyanic acid on the blood of vertebrate animals may not depend on its exerting a decomposing action

on the constituents of the blood similar to that seen in the case of alloxan.

Riche* has investigated the decomposition of some bibasic acids. When suberic acid is heated with excess of baryta, vapours are given off which condense into a pale yellow liquid. On rectifying this, it is found to consist principally of a liquid which boils at 76°, the analyses and vapour-density of which lead to the formula $C^{12} H^{14}$. Its formation may be thus expressed :—

$$C^{16} H^{12} O^6, 2HO + 4BaO = C^{12} H^{14} + 4BaO\ CO^2.$$
Suberic acid.   Hydrocarbon.
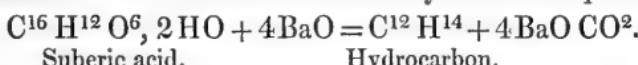
It is a mobile, highly refracting liquid, of specific gravity 0·671. It is converted by chlorine into a viscous mass with disengagement of hydrochloric acid. With bromine, iodine, nitric acid, and sulphuric acid no definite results are obtained.

Sebacic acid distilled with excess of baryta behaves in a similar manner; the reaction is very energetic, and it is necessary to mix the mass with a quantity of sand. The liquid product, when redistilled, consists of a hydrocarbon boiling at 127° C. It is a colourless, highly aromatic liquid, and burns with a blue-edged flame. Treated with chlorine it becomes thick, and disengages hydrochloric acid. Its analyses and vapour-density give for it the formula $C^{16} H^{18}$. It appears to contain a little of the body $C^{16} H^{16}$.

The formation of the hydrocarbon is thus :

$$C^{20} H^{16} O^6, 2HO + 4BaO = C^{16} H^{18} + 4BaO\ CO^2.$$
Sebacic acid.   Hydrocarbon.
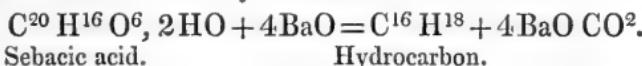
From their composition these hydrocarbons would belong to the series of homologues of marsh-gas, $C^n H^{n+2}$. They are probably, however, only isomeric. The hydrocarbon next below the one from suberic acid, hydride of amyle $C^{10} H^{12}$, boils at 31°; while the hydrocarbon, $C^{12} H^{14}$, boils at 76°, and $C^{16} H^{18}$ boils at 127°. It is probable that this series of bibasic acids will yield a series of hydrocarbons isomeric with those from the monobasic fatty acids.

XXXVIII. *On Luminosity of Meteors from Solar Reflexion.*
By R. P. GREG, *F.G.S.*†

IT has as yet by no means been decided from what cause arises the luminosity of shooting-stars. I do not now propose to consider in detail all the various theories relative to this subject, but shall endeavour to prove that their luminosity cannot at least arise from solar reflexion, a theory partially supported by

Sir J. Lubbock and others. The very sudden appearance and disappearance of shooting-stars and small meteors, and their general resemblance on a small scale to comets which shine by solar reflexion, certainly favour the idea, either that suddenly entering the cone of the earth's shadow they are instantly eclipsed, or conversely, become visible as they emerge from it; or *secondly*, previously self-luminous in planetary space, they may become suddenly extinguished on entering the denser atmosphere of the earth; or *thirdly*, they may suddenly become visible and luminous, only on entering the earth's atmosphere by friction and compression, by rapid absorption of oxygen and sudden chemical action, or by electrical excitation.

I shall consider the first supposition most fully and in the first instance, because I consider it may be most readily and completely disproved. Sir J. Lubbock, in an interesting paper in this Magazine for February 1848, and one that has since been frequently referred to, considers the hypothesis of solar reflexion as a very applicable one in certain varieties of shooting-stars: he even says, "knowing the time when, and the place where the star disappeared, the elements of the geometry of three dimensions furnish the means of determining the exact distance of the body from the place of the spectator or from the centre of the earth;" and in his paper he gives several geometrical equations and formulæ for assisting such determinations. I do not propose entering into the nature of these calculations, or to question either the results or the *data,* but merely by a different treatment to show, if I can, how unlikely, if not impossible, it is that ordinary shooting-stars (I mean, of course, those not showing symptoms of active ignition within the lower limits of the earth's atmosphere) can ever shine by reflected solar light; and this simply from the fact that they would be quite too far off for us to observe such small bodies, at even the *minimum* distance at which (at certain times and places on the earth's surface when and where we know they are very frequently seen) they actually could be so visible.

The problem I propose then to solve is, what is the *minimum* distance at which a shooting-star could be thus visibly luminous seen at an angle, say of 45 degrees above the horizon (the majority of shooting-stars appearing, as a rule, to the spectator at even a greater angle), to an observer situate at midnight within the tropics, or, to be more precise, at the equator, about the time of the vernal or autumnal equinox.

In fig. 1, let S be the centre of the sun, and E that of the earth, and S A and E C semi-diameters of sun and earth respectively; let $SE = 95,000,000$ miles, B S and $CE = 3950$ miles, and $SA = 422,500$ miles; then supposing the shadow of the

earth to form a true cone $a \, O \, a'$, on the principle of similar triangles we have the proportion

$$B \, A = 418,550 : B \, C = 95,000,000 :: E \, C = 3950 : E \, O$$
$$= 855,660 \text{ miles,}$$

Fig. 1.
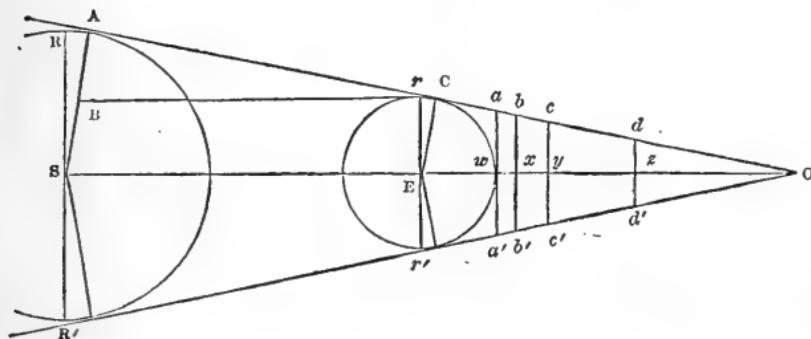
the length of the axis of the shadow from the centre of the earth. And the diameters $a \, a'$, $b \, b'$, &c. are in proportion to the distances $O \, w$, $O \, x$, &c., as the diameter $r \, r' = 7900$ miles to the whole length $E \, O$, &c. Now $E \, C = E \, w = 3950$ miles; and let $w \, x = 200$ miles, then we have

$$855,660 : 7900 :: O \, x = 851,510 : 7862 \text{ miles} = b \, b';$$

similarly, $a \, a' = 7863\frac{1}{2}$ miles; and calling $w \, z = 8000$ miles, we have $d \, d' = 7790$ miles; and if $w \, y = 3931$, then $c \, c' = 7831$ miles.

I have taken $w \, x = 200$ miles, that being considerably over the average distance of shooting-stars whose distances have been pretty accurately determined by Heiss, Brandes, Benzenburgh, Twining, and Quetelet; w is the situation of the supposed observer at midnight, near the equator at the time of the vernal or autumnal equinox. From the above it will be seen that at a distance of 8000 miles from the spectator at w, the cone of the earth's shadow or *umbra* would have a breadth of not less than 7790 miles.

On referring now to fig. 2, which is merely a portion of fig. 1 enlarged for the sake of convenience, it will be easy to ascertain the *minimum* distance $w \, c$ at which a shooting-star m could be visible outside the cone of shadow to a spectator at w, the angle $a \, w \, y$ being $= 90°$, and $w \, a = w \, y = 3931$ miles; then as $c \, y$
$$= \frac{7831}{2} = 3915\frac{1}{2} \text{ miles,}$$
$$\therefore \; w \, c = \sqrt{(3915^2 + 3931^2)} = 5547 \cdot 97 \text{ miles,}$$

the angle $a \, w \, c$ being consequently just over $45°$, *i. e.* a distance,

as far as we know, far too great to admit of our seeing ordinary shooting-stars, at least in countries within the tropics, and if shining by reflected solar light.

Fig. 2.
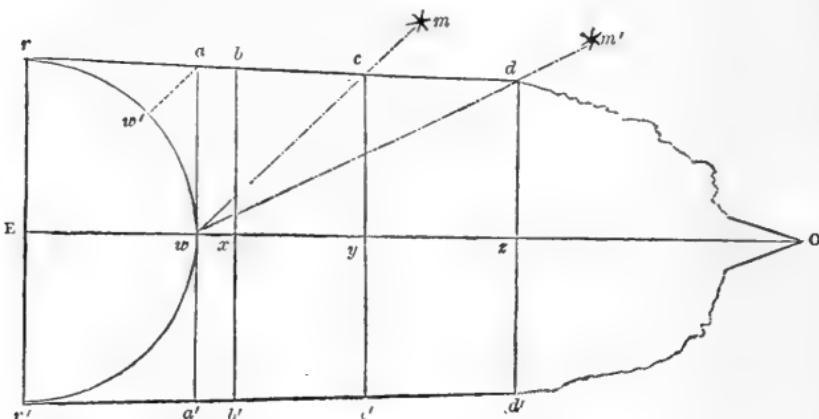
The distance to the outside of the *umbra* in latitude 45° half-way between w and r, as at w', would certainly be considerably less than 5547 miles; though this would not vitally affect the question, as the distance towards c' would proportionately increase; and $w' a$ would still be over 1500 miles at midnight.

If the altitude of the meteor be, say 64°, dz being $= \dfrac{7790}{2}$

$= 3895$ miles, then $w d = 8900$ miles; and if seen in the zenith at O, its distance would not be less than 871,710 miles, as seen from w, the supposed centre of the cone of shadow.

Now as the average distance of shooting-stars at the time of their visibility is not much more than 100 miles, varying from 50 to 150 miles, and as we usually see them in all quarters of the heavens at an average elevation of at least 45 degrees, it is pretty clear, I think, if the preceding calculations and suppositions are correct, that the majority of the shooting-stars we see do not shine by reflected solar light.

I may at a future time offer some observations on other and more probable causes of luminosity in meteors and shooting-stars, and in the mean time refer those interested in this subject to the last Report of the British Association for Leeds in 1858, by the Rev. Baden Powell. The theory of Mr. Daniel Vaughan, alluded to in that Report, deserves attention.

XXXIX. *On a Carbonate of Lead from Leaden Coffins.* By
Richard V. Tuson, *Lecturer on Chemistry at Charing Cross
Hospital**.

ABOUT twelve months ago an Order of Council was issued
directing the coffins in the vaults of the church of St. Mar-
tin's in the Fields to be transferred to the catacombs.   A few
days after the appearance of this order, my friend and colleague
Mr. Canton, in company with several other gentlemen, visited
the vaults with the view of endeavouring to find the remains of
the late celebrated surgeon, John Hunter, which were known to
have been deposited there.   The search proved successful, and
Hunter's remains were subsequently reinterred in Westminster
Abbey.

During his visit, Mr. Canton observed that many of the leaden
coffins, although they retained their original shape, were, with
the exception of an external and exceedingly thin plate or foil of
metal, converted into an earthy-looking substance.   Several
pieces of this substance were removed from a coffin which, there
is good reason for believing, had been in the vaults about eighty
years.   These were placed at my disposal; and although it was
thought that they principally consisted of carbonate of lead, it
was nevertheless considered, from the peculiarity of the circum-
stances under which the material was formed, that the results of
its analysis might prove somewhat interesting.

The pieces of the substance referred to were about a quarter
of an inch in thickness: they had a laminated structure, and
possessed a fawnish or drab-white colour.   Neither crystalline
form nor metallic lead were detected even by the aid of the
microscope.   The material was tolerably brittle, and readily
reduced to an impalpable powder.   On submitting it to quanti-
tative analysis, the following were the results obtained:—

$$
\begin{array}{lr}
\text{Moisture} & 0 \cdot 10 \\
\text{Organic matter and loss.} & 0 \cdot 52 \\
\text{Peroxide of iron} & 1 \cdot 94 \\
\left.\begin{array}{l}\text{Protoxide of lead} \\ \text{Carbonic acid}\end{array}\right\} \begin{array}{r}82 \cdot 29 \\ 15 \cdot 15\end{array} & = \left\{\begin{array}{l}\text{PbO, CO}^2 \quad 92 \cdot 28 \\ +\text{PbO} \qquad 5 \cdot 16\end{array}\right. \\
\hline
100 \cdot 00 &
\end{array}
$$

The results of the analysis of this substance, therefore, show
that it chiefly consists of protocarbonate of lead with a small
proportion of anhydrous protoxide of the same metal.   The pro-
duction of these compounds was doubtless mainly due to the

* Communicated by the Author.

moisture and carbonic acid evolved during the decay of the animal remains, acting, conjointly with the oxygen of the air, on the leaden coffins in which the bodies were placed.

If one might venture to assign a formula to this mixture of carbonate and oxide of lead, its composition would be represented by $PbO + 15(PbO, CO^2)$, as the following numbers clearly indicate :—

	Calculation.	Experiment.
$16 PbO = 1785\cdot6$	$84\cdot41$	$84\cdot45$
$15 CO^2 = 330\cdot0$	$15\cdot59$	$15\cdot55$
	$100\cdot00$	$100\cdot00$

The interesting points in connexion with this substance are, that it is anhydrous, that it contains but a small excess of oxide, and that it consequently differs in composition from any of the carbonates of lead hitherto described as being produced by the united action of air and water on metallic lead ; or by the influences concerned in the well-known Dutch method for manufacturing "white lead," and which, it is believed, approximate in character to those under which the material forming the subject of this communication was developed.

The difference in composition of the various carbonates of lead formed under the circumstances referred to, will be seen by glancing at the subjoined Table :—

Source.	Composition.
Air and water on lead	$PbO, HO + PbO, CO^2.$
Dutch method	$PbO, HO + 2(PbO, CO^2),$ and sometimes $PbO, HO + 3(PbO, CO^2).$
Leaden coffins	$PbO + 15(PbO, CO^2).$

Were any of these hydrated and basic carbonates of lead exposed sufficiently long to the action of carbonic acid, they would in all probability be transformed into perfectly neutral and anhydrous carbonates.

Lastly, it is most likely that the lead of the coffins was first converted into hydrated oxide, then into hydrated and basic carbonate, and finally into the anhydrous carbonate of the composition already given.

March 21, 1860.

XL. *On Osmious Acid, and the position of Osmium in the list of Elements. By* J. W. Mallet, *Professor of Chemistry, &c., University of Alabama**.

IN most chemical text-books it is stated, on the authority of Berzelius, that there are five oxides of osmium—OsO, $Os^2 O^3$, OsO^2, OsO^3, and OsO^4—of which, however, the second and fourth have not been isolated, although compounds containing them are known. To these may be added a blue substance, first obtained by Vauquelin and supposed by Berzelius to consist of OsO united to either $Os^2 O^3$ or OsO^2, and the highest oxide, probably OsO^5, the existence of which was announced by Fremy in 1854.

While preparing osmium from some black platinum residues, I have accidentally obtained a substance which there is some reason to believe may be osmious acid—the hitherto unisolated teroxide—mixed indeed with osmic acid, but still permitting certain of its properties to be observed.

Three or four ounces of the platinum residue were treated by a modification of the original process of Wollaston, now seldom adopted. The powder was mixed with three times its weight of nitre, the mixture was fused for some time in an iron crucible, and then poured out upon an iron plate. While still warm the fused cake was broken into fragments and put into a flask fitted with a cork, through which passed a tube two feet long, bent at right angles, and a funnel-tube, the latter drawn out to a very small bore at the lower end, and reaching to the bottom of the flask. The bent tube was well cooled, and *undiluted* oil of vitriol was very cautiously poured, by a few drops at a time, into the funnel.

The acid produced intense heat on coming in contact with the cake of potash salt, and oily drops of a bright *yellow* colour began to make their appearance in the cooled tube. These drops very slowly congealed to a solid resembling unbleached bees-wax. By the time the sulphuric acid had been added in slight excess, a considerable quantity of this yellow substance had collected in the tube and in a receiver attached. By gently heating, the whole was obtained in the receiver, and united under a little water to a single mass. Towards the end of the distillation *colourless* needles and fused drops of the well-known osmic acid came over, and doubtless a considerable portion of the yellow mass in the receiver consisted of the same.

At first it seemed probable that the yellow colour of the latter was due merely to some impurity, and it was therefore cautiously

resublimed, but it again collected of the same tint as before. It appeared to be even more fusible and volatile than osmic acid; t took a long time to congeal under a stream of cold water flowing over the outside of a tube in which it had been melted.

The water in which it was fused acquired a bright yellow colour, and gave off fumes, the odour of which seemed to me somewhat different from that of osmic acid, and which irritated the eyes so insufferably that it was scarcely possible to finish work with the acid and put it up for preservation. It was removed as a single cake from the water, and sealed up hermetically in a glass tube which had been previously cleansed with care from all traces of dust or other organic matter. The water in which it had been fused was mixed with caustic potash, and gave a solution of very dark brown-red colour, such a tint as would probably result from a mixture of the red* *osmite* of potash discovered by Fremy with the orange-brown *osmiate* of potash.

The sealed tube containing the fused cake or stick of yellow acid was allowed to remain upon a table exposed to the direct rays of the sun. The acid immediately began to sublime upon the sides of the tube, not in long needles and prismatic crystals like osmic acid (which seems to be monoclinic), but in feathery crusts like sal-ammoniac, which under a lens had somewhat the appearance of minute octahedrons grouped together. The colour was still bright yellow, but in a short time the sublimed acid began to turn black, and in twenty-four hours the whole inner surface of the tube was perfectly black and opake. A tube containing pure colourless osmic acid has been exposed in a similar way to the sun for three weeks without any such blackening taking place. A tube closed by a cork, or one from which dust has not been carefully removed, will often cause osmic acid to turn dark, but never exhibits anything like the absolute blackness and opacity of the whole tube noticed in the present instance.

It is easy, however, to imagine the cause of this change undergone by the yellow acid if it be in fact the teroxide of osmium (mixed with osmic acid). The teroxide probably broke up into osmic acid, and one of the lower oxides of osmium, or perhaps the metal itself. We might have

$$2 OsO^3 = OsO^4 + OsO^2,$$
$$5 OsO^3 = 3 OsO^4 + Os^2 O^3,$$
$$3 OsO^3 = 2 OsO^4 + OsO,$$
or
$$4 OsO^3 = 3 OsO^4 + Os.$$

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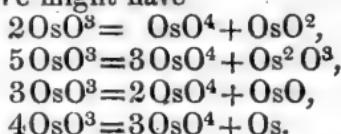
* A rose-red colour is also characteristic of the salt supposed by Berzelius to be the ammonio-*terchloride* of osmium, corresponding in the chlorine series to *osmite* of ammonia.

In order to ascertain, if possible, which of the above changes had taken place, the tube was opened two or three months after it had been sealed, and the contents were examined. The fused stick of acid was found to be black and partially friable; on heating in another glass vessel, most of it sublimed, leaving a little black powder behind, and condensed in needles, still slightly yellowish, but differing little in appearance from common osmic acid. The inner surface of the original tube was found coated with a thin, filmy, adherent crust, of a black colour and considerable lustre. This was scraped off, and a portion of it gently heated in a stream of dry carbonic acid gas until all traces of adherent osmic acid were driven off. After cooling, the carbonic acid was replaced by dry hydrogen, and heat was again applied. Water condensed on the tube beyond the heated part, thus proving that an oxide of osmium, not the metal, was under examination. Replacing again the hydrogen by oxygen, osmic acid was produced and carried off with the stream of gas. The black powder scraped off from the original tube was heated with hydrochloric acid, and seemed to be but slowly acted on; the acid, however, assumed a green colour, and hence it is probable that the osmium existed as protoxide.

It is not easy to see, without further investigation, how osmious acid could have replaced in part osmic acid in the attempt to prepare the latter as above described. Is there a particular stage of the decomposition of nitre by heat at which osmium may replace nitrogen in nitrite of potash (KO, NO^3)? From the relations of the two elements, to be noticed presently, this would seem probable, and in fact Fremy has noticed the crystallization of osmite of potash from a solution in hot water of the fused cake of nitre and iridosmium. A reason for osmic acid (OsO^4) being usually obtained from the latter, instead of osmious (OsO^3), might perhaps be found in the fact that the chemists who of late years have worked upon osmium recommend the use of nitric or nitro-muriatic acid to neutralize the potash; sulphuric acid, to which Wollaston had recourse in his early experiments, is now seldom employed. Thomson, in his 'Chemistry of Inorganic Bodies,' published many years ago, observes that osmic acid has sometimes a tint of yellow.

It does not seem likely that the cork closing the neck of the flask used for distillation had anything to do with the production of osmious acid, if such took place; the cork itself did not show any appearance of being acted on, and there was no blackening of its surface until some time after the experiment was ended.

The reduction of osmic acid generally results in the formation of the basic oxides; Berzelius, however, observed that on adding sulphurous acid to a solution of osmic acid, the latter passed

through various shades of colour—*yellow, orange-yellow,* brown, green, and at last blue; he attributed these tints to the successive formation of sulphates of the binoxide, sesquioxide, and blue oxide: but may not the first step in the reduction have been osmious acid, giving the yellow colour?

Another and altogether different view of the nature of the volatile yellow substance above described was suggested as possible by some remarks of Claus, in a recent paper " On the Tendency to Reduction of Salts of Iridium " (*Ann. der Chem. und Pharm.* August 1858, p. 129). This author has shown that the platinum metals fall naturally into three groups, in each of which are contained two metals resembling in general habit and relations each other more closely than members of the remaining groups. Platinum and palladium constitute the first of these pairs, iridium and rhodium the second, osmium and ruthenium the third. The atomic weight of the first-mentioned member of each pair is higher than (nearly double) that of the second.

In the paper quoted, Claus remarks that the metal of lower atomic weight in each of these groups is much more easily reduced than the other from superior to inferior grades of combination with chlorine: thus the bichloride of palladium is reduced with much greater ease to protochloride than is the corresponding compound of platinum; and for the same reason, probably, the bichloride of rhodium is not known, but only the sesquichloride, while both salts of iridium can be easily obtained. On this same principle Claus explains the fact that no oxide of ruthenium homologous with osmic acid has been obtained, while he gives the following reasons for suspecting the existence of such an oxide:—" This opinion is based upon the fact, that in my preparation of compounds of ruthenium, which can be obtained only by energetic processes of oxidation, the material worked upon, notwithstanding all my care and economy, gradually diminished, and yet I have never succeeded in collecting a volatile product.   Once only, when I had fused ruthenium perfectly free from osmium, with caustic potash and nitre, dissolved the mass in water, and decomposed it with nitric acid, I observed a peculiar odour, quite distinct from that of osmic or nitrous acid; and afterwards, having covered the beaker, which was smeared on the edge with tallow, with a plate of glass, I remarked an unmistakeable blackening of the tallow, caused by the reduction of a volatile metallic compound."

It seemed possible that the volatile yellow substance to which the present paper refers might have been an acid oxide of ruthenium[*]—RuO^3, RuO^4, or RuO^5—and reducible with extreme

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[*] If such a compound exist, an explanation may be found for the process by which Fremy has obtained a lower oxide of ruthenium (probably the

facility, Claus and others having already noticed the reducing effect of light upon salts of the platinum metals. A portion of the crust of yellow acid from the sides of the tube was carefully examined for ruthenium, the various tests given by Claus, as well as that recently proposed by Dr. Gibbs, being made use of; but no proof of the presence of this metal could be obtained.

The properties of osmium and its compounds are very remarkable, and render it a matter of no little interest to trace the analogies of this rare substance and fix its place among the other elements. It is described in most chemical works along with platinum and its associated metals,—mainly on the ground of community of origin; for in many respects it is unlike the platinum, palladium, rhodium, &c., with which it always occurs in nature. All these metals are commonly thought of as very infusible, of great density, very slightly affected by reagents, and very easily reduced from their compounds to the metallic state; when more closely examined they are found to differ from each other in many of their other properties. The arrangement by Claus of the platinum metals in three groups, each containing one metal of high and one of low atomic weight, viz.

| Platinum, | Iridium, | Osmium, |
| Palladium, | Rhodium, | Ruthenium, |

has been alluded to above; the two members of each group are more closely related to each other than to any of the rest. Osmium and ruthenium are clearly the most electro-negative of the series. Graham has inferred the isomorphism of platinum, palladium, iridium, and osmium, from the fact that their potassio-chlorides all crystallize in the form of the regular octahedron: the corresponding compound of ruthenium has since been added to the list, while that of rhodium is still unknown. The occurrence of two salts under the same form, *in the regular system,* of course does not of itself suffice to establish the relation of isomorphism between them; iridio-chloride of potassium seems how-

binoxide) in crystals. He roasts the powder of platinum-residue in a stream of air drawn through a porcelain tube at a bright red heat; osmic acid volatilizes, and is said to carry with it *mechanically* the oxide of ruthenium, which deposits upon fragments of porcelain placed in the cooler part of the tube. But the oxide is in distinct crystals, and can therefore scarcely be conceived of as a powder borne along in a merely mechanical way by a stream of vapour; and, moreover, there is no reason for oxide of ruthenium only being so borne along, while other substances of no greater density remain behind. Is it not more likely that a volatile and very easily reducible homologue of osmic acid is formed, and almost immediately afterwards decomposed, depositing the binoxide of ruthenium?

ever to be capable of crystallizing in all proportions with the platino- and osmio-chlorides.

The interesting fact has been discovered by Claus, that osmio-cyanide and ruthenio-cyanide of potassium are strictly isomorphous with the well-known ferrocyanide, crystallizing with it in all proportions, and even giving very similar precipitates with various metallic solutions; so that, in these double cyanides, osmium and ruthenium are capable of taking the place of *iron*.

In the greater number of its relations, however, osmium presents itself as a member of the *arsenic group* of elements. This has been noticed by some recent authors, as by Prof. Dana in the arrangement of the elements adopted in his 'System of Mineralogy,' and by Prof. Miller, who says in his lately published 'Elements of Chemistry,' that "it presents more analogy with arsenic and antimony than with the noble metals." Fremy, too, compares osmium in platinum ore to arsenic in the native arseniurets.

Nitrogen, phosphorus, arsenic, antimony, and bismuth are generally recognized as forming a distinct and natural group of elements; and into this group it seems from many considerations that osmium, and probably ruthenium, ought to be introduced. They have some analogies with other natural families, just as arsenic is allied to sulphur in native sulph-arseniurets, and nitrogen and chlorine exhibit some resemblance in the nitrates and chlorates; but here appear to be their closest relations. It may be interesting to notice some of the principal points of resemblance to or difference from this group.

Iridosmine occurs in crystals closely related in form to those of arsenic, antimony, and bismuth in the metallic state. The analyses of iridosmine are not yet sufficiently numerous or accurate to enable us to decide upon its normal composition; but it seems probable that the two metals occur in variable proportions, and are in this mineral isomorphous, thus establishing, as noticed by Dana, a connexion between the arsenic group and that of the distinctly basic metals, as the arsenic and sulphur groups are united through homœomorphous bismuth, tetradymite and tellurium. Dana places iridium in the same section with iron, among the metals whose most stable grades of oxidadation are the protoxide and sesquioxide; but the statement of Claus, that the *binoxide* of iridium is the most stable and easily prepared compound with oxygen, would remove this metal, as also perhaps platinum and palladium, from the iron section to that containing tin and titanium; and the propriety of this transfer may be supported by the relationship of Fremy's crystallized oxide of ruthenium (doubtless the binoxide) examined by Senarmont: this was found to be homœomorphous with

stannic and (the rutile form of) titanic acid. The bichloride of tin and potassium, too, is reported as crystallizing in regular octahedrons, like the corresponding salts of iridium, platinum, and palladium.

The arsenic section, as given by Dana, includes nitrogen, phosphorus, arsenic, antimony, bismuth, osmium, and tellurium. The last-named is marked as doubtful, and should decidedly be placed with sulphur and selenium, to which it is analogous in by far the greater number of its compounds.

In one of the interesting memoirs lately published by Dumas, on the numerical relations subsisting among the atomic weights of the elements, the arsenic series is thus given :—

		Atomic weights.
Nitrogen	. . . .	14
Phosphorus	. . . .	$14+17=31$
Arsenic	. . . .	$14+17+44=75$
Antimony	. . . .	$14+17+88=119$
Bismuth	. . . . .	$14+17+176=207$

and the parallelism of this series with that of chlorine, iodine, &c., is supposed to be shown in the following lines :—

F (19)	Cl (35·5)	Br (80)	I (127)
N (14)	P (31)	As (75)	Sb (122)

in which a common difference of 5 is assumed between the two members in each of the vertical columns (a difference not strictly brought out in the case of phosphorus and chlorine), and in which a higher atomic weight is assigned to antimony than in the preceding Table. Osmium is not included ; but in a supplemental note since published, we find it placed, with an equivalent somewhat higher than that usually adopted, in the *sulphur* group, serving to complete the following two lines of equivalents :—

Mg (12·25)	Ca (20)	Sr (43·75)	Ba (68·5)	Pb (103·5)
O (8)	S (16)	Se (39·75)	Te (64·5)	Os (99·5)

between the paired members of which a common difference of 4 is supposed to exist.

Let osmium and ruthenium be brought into the arsenic group, and the series of atomic weights will then stand thus :—

		Atomic weights.
Nitrogen	. . . .	14
Phosphorus	. . .	$14+17=31$
Ruthenium	. . .	$14+17+22=53$
Arsenic	. . . .	$14+17+44=75$
Osmium	. . . .	$14+17+66=97$
Antimony	. . . .	$14+17+88=119$
Bismuth	. . . . .	$14+17+176=207$

The atomic weights of ruthenium and osmium are here assumed as 53 and 97; numbers not differing more widely from those commonly received—52·2 (Claus) and 99·6 (Berzelius)—than do several of those assumed by Dumas. Our knowledge of these two equivalents is based upon very limited data, and can but be looked on as merely approximative. As regards osmium, Fremy says that in several experiments he has obtained an equivalent number lower than that given by Berzelius; and the vapour-density of osmic acid, which we shall notice presently, points to an equivalent close to 97. A redetermination of this equivalent is very much to be desired.

Taking the series as given above, we find ruthenium and osmium to fall in between phosphorus and arsenic, arsenic and antimony,—the numbers from phosphorus to antimony increasing by 22—44—66—88, just as in the following group given by Dumas:—

		Atomic weights.
Chromium	. . . .	26
Molybdenum	. . . .	$26 + 22 = 48$
Vanadium	. . . .	$26 + 44 = 70$
Tungsten	. . . .	$26 + 66 = 92$

and we may arrange the two series in parallel lines,

P (31)	Ru (53)	As (75)	Os (97)
Cr (26)	Mo (48)	V (70)	W (92)

These numerical relations are of very little importance in themselves, when we employ the small numbers of the hydrogen scale of equivalents, and especially when we permit ourselves to alter the numbers themselves to any extent, however small; but they acquire more interest when they present us with groupings of elements which we acknowledge on other grounds to be naturally related. In such cases, when the homology is distinctly marked, we may even be justified in taking some liberties for the moment with the numbers standing, often with but slender evidence to support them, for the equivalents of the less-known elements; and we may, perhaps, thus be directed to errors of determination which future experiments will clear away.

The bodies named in each of the two lines just given are homologous in many respects besides that of atomic weight, and a connexion between the two series, through vanadium, has lately been shown by Schafarik. There is a clear resemblance running through the formulæ and properties of their oxides. In the chromium series—a very natural one—the most important oxides are the metallic acids of the composition MO^3; we have also in each case a binoxide, MO^2; but the sesquioxide is pro-

minent only in the case of chromium itself, and indicates the relation of this metal with iron.

In the arsenic series the known oxides are the following:—

	$P^2 O$					
NO	PO	RuO	AsO (?)	OsO	SbO (?)	BiO (?)
		$Ru^2 O^3$ (?)		$Os^2 O^3$ (?)		
NO^2		RuO^2		OsO^2		
NO^3	PO^3	RuO^3	AsO^3	OsO^3	SbO^3	BiO^3
NO^4	PO^4 (?)			OsO^4	SbO^4	BiO^4
NO^5	PO^5		AsO^5	OsO^5	SbO^5	BiO^5(?)

The prominent compounds in the Table are the acids MO^3 and MO^5; with respect to the separate columns, the following facts are noticeable.

The oxides of nitrogen are well known; the regularity observable in this column causes it to be frequently used as an illustration of the "law of multiples." NO and NO^2 are usually said to be neutral; but the latter plays the part of a base in contact with sulphuric acid, as in the crystals of the oil of vitriol chambers, and possibly the former may do so, too, in the nitro-sulphates (KO, NO, SO^3 and NH^4O, NO, SO^3?) obtained by Davy by bringing nitric oxide in contact with an alkaline sulphite. NO^3 and NO^5 are well-known acids. It is doubtful whether hyponitric acid (NO^4) is capable of combining with bases and forming salts; in contact with the alkalies it yields a mixture of nitrites and nitrates, yet, *when out of contact of bases, it seems to be a body of more stability than either* NO^3 *or* NO^5 (*anhydrous*).

In the column of the oxides of phosphorus, we have first the very anomalous suboxide ($P^2 O$), which is probably the only marked exception to the homology running through the whole table. Before the discovery of red (amorphous) phosphorus by Schrötter, this substance was, no doubt, to some extent confounded with phosphoric oxide, and may even now throw some doubt upon the cases in which the latter seems to have been obtained pure and to have yielded a formula supported by trustworthy analyses. PO, unlike the other protoxides of the series, is usually considered an acid; but as it has not been obtained in the separate state, and all the hypophosphites contain water, it may be reasonably assumed that the formula of the acid should include hydrogen. PO^4 is doubtful: this may, perhaps, be the composition of Pelletier's phosphorous acid, produced by the slow combustion of phosphorus, a body which undergoes no further oxidation by prolonged exposure to the air, and which, in contact with bases, yields mixed phosphites and phosphates. The last term in the column, phosphoric acid, is well known.

The existence of a distinct protoxide of arsenic, as of antimony and bismuth, is doubtful.   Arsenious acid is a *feeble, volatile, metallic acid*—feebler in its relations as an acid than arsenic acid, and volatilizing at a lower temperature than the latter.   Arsenious acid, moreover, volatilizes at a temperature below that required by metallic arsenic.

In the antimony column, the oxide SbO^3 is usually viewed as a weak base, but seems also to be capable of uniting as a feeble acid to the alkalies, and even of expelling carbonic acid from their carbonates (Liebig).   The isodimorphism of SbO^3 and AsO^3 is well established. SbO^3 is volatile at quite a moderate temperature, while metallic antimony requires at least a white heat to vaporize it. SbO^5 is a body of distinctly acid properties. *Both* SbO^3 *and* SbO^5 *are converted by heating in the air into* SbO^4 —the so-called antimonious acid, which seems therefore to be the most stable oxide when strong bases and acids are not present.   It is most probable that, as Fremy maintains, SbO^4 is not itself an acid, but that a so-called alkaline antimonite is, in fact, a mere mixture of an antimoniate with the compound of antimonic oxide and alkali $(2SbO^4 = SbO^3 + SbO^5)$.

In the bismuth column, the teroxide is homologous as a base with teroxide of antimony, but shows little tendency to play the part of an acid with even the strongest bases.   This oxide and the metal itself are volatile at high temperatures. BiO^4 also seems to be devoid of acid properties; but the compound BiO^5 probably exists, and is homologous with SbO^5, forming alkaline salts of little stability.

Comparing now ruthenium and osmium with the above recognized members of the arsenic group, we find, first, that both metals form protoxides, which are feeble bases, as are probably the corresponding compounds of the other members of the group. We next meet with the sesquioxides, whose formula is exceptional in the series; but for neither metal has this grade of oxidation been obtained in the free state and pure, and in the case of osmium its existence may be gravely doubted.   Anhydrous $Ru^2 O^3$ is supposed by Claus to be formed during the roasting of metallic ruthenium in the air at a high temperature; but only on the ground that the absorption of oxygen slackens when *about* enough has been taken up to form this compound, and that the proportion necessary for the binoxide is never fully attained. Claus, however, describes a sesquichloride with which double salts are formed by the chlorides of potassium and ammonium, and we must therefore assume a sesquioxide also.   Sesquioxide of osmium is quite unknown in the separate state; and the belief in its existence is founded solely upon the preparation by Berzelius of a dark brown substance, supposed to consist of the

sesquioxide united to ammonia, which, dissolved in hydrochloric acid, yields a brown compound, supposed to be the sesqui-chloride of osmium and ammonium.   Neither of these, however, can be crystallized, nor has the constitution assigned to either been supported by an analysis.   The so-called ammonio-sesqui-oxide detonates when heated (sometimes with much violence, as I have noticed in removing by heat the deposit of this substance which forms on the end of a retort-neck during the distillation of osmic acid into a receiver containing ammonia), and hence is probably analogous to fulminating platinum, containing perhaps the binoxide of osmium.   The binoxide itself is a feeble base, the characteristic colour of whose salts in solution is yellow, as is the case with the corresponding compounds of iridium.   Similar remarks apply to the binoxide of ruthenium—probably the body obtained, as we have shown, by Fremy in crystals.   The teroxide of osmium is the body supposed to have been isolated in the experiment described at the beginning of this paper.   Its position as a feeble acid, capable, however, under some circumstances, of playing the part of a base, its fusibility and volatility (greater apparently than those of osmic acid, as nitrous acid is more fusible and volatile than hyponitric), its probable crystallization in octahedrons of the regular system, in which arsenious acid and teroxide of antimony are also found, all tend to indicate homology with the other teroxides of the arsenic group.   The general relations of ruthenic acid, so far as these are known, place it in a similar position.   Just as we find hyponitric acid (NO^4) and antimonious acid (SbO^4) to be the most stable of the higher oxides of nitrogen and antimony, so the well-known osmic acid (OsO^4) seems to be the grade of oxidation which osmium most readily assumes and retains when not in contact with bases. OsO^3 and OsO^5 (the latter as described by Fremy) seem scarcely capable of existing in the separate state ; when set free from their salts they soon pass into OsO^4; while it may as well be doubted that the latter ever exists as a distinct acid in combination with bases, as that NO^4 or SbO^4 does so.   No so-called osmiate has ever been analysed ; the saturating capacity of the acid, if it be such, is unknown ; when free and in solution in water, it has no acid reaction ; it does not displace carbonic acid from the carbonates, and it is itself expelled by heat from most of its supposed compounds, and is separated in part by water even from potash and soda.   No compound of OsO^4 with a base has been obtained in crystals, while Fremy states that he has crystallized the alkaline salts of both OsO^3 and OsO^5. RuO^4 and RuO^5 are as yet unknown.

The tendency throughout the whole arsenic group is manifestly to the production of the acid compounds MO^3 and MO^5;

the former the more fusible and volatile body, the latter the stronger acid. In addition, we have some cases of the protoxide (MO), a feeble base, and the binoxide (MO²), a body of still more feebly basic properties, verging upon the acids. All other grades of oxidation, so far as they exist at all, may perhaps be correctly viewed as compounds of the preceding *inter se.* The stability of the oxide (MO⁴) in the separate state is remarkable; its formula is one of rare occurrence.

The affinity of all the elements of the group for oxygen is considerable; it is so even in the case of osmium and ruthenium, usually placed among the noble metals. Dumas (*Traité de Chim. app.*) states that osmium does not oxidize at common temperatures, nor even at 100° C.; but I have obtained conclusive evidence that oxidation may go on slowly even at the ordinary atmospheric temperature. The paper label and the cork of a tube containing pure metallic osmium have in the course of several years become blackened, precisely as organic matter is by the fumes of osmic acid, the black tint on the paper decreasing from the mouth of the tube along the outside. A piece of white paper, in which some black platinum residue had been wrapped, was strongly stained in the immediate neighbourhood of the powder in the course of a few weeks. *The same effect is distinctly observable even upon the paper label placed inside a tube of native iridosmine* (Siberian) *in the usual coarse grains*—a specimen which has lain among other minerals, and has never been placed near any artificial preparations of osmium. Osmium, like arsenic and antimony, is clearly capable of slowly taking up oxygen at common temperatures. At a red heat, roasting in a current of air affords, as is known, a good method of obtaining osmic acid from the iridosmine of platinum residues—just as by similar roasting arsenious acid is prepared from the native arseniurets.

It would be a matter of much interest to compare osmium with its supposed homologues under circumstances in which we should expect it to play an electro-negative part. Fremy has announced his belief in the existence of an *osmiuretted hydrogen*; but such a body has not yet been isolated and described. Compounds of the metal with ethyle, methyle, &c., would be well worth examination; and it is not unlikely that such might be prepared from a body which in some states of combination exhibits such a high degree of volatility.

The earlier experiments of Deville and Debray upon the platinum metals seemed to have shown that both osmium and ruthenium could be volatilized, at exceedingly high temperatures, without previous fusion; if this were confirmed, a strong point of resemblance with arsenic would be made out; but it appears from a more recent paper, that osmium at least may be fused and

obtained as a perfectly compact mass, the apparent volatility of the metal being due doubtless to previous oxidation, the crucibles used being permeable to air. We have seen, as regards arsenic and antimony, that their oxides are more volatile than the metals themselves.

It was lately stated that osmium may be obtained in crystals by the same means as those used for boron and silicon, but I have as yet seen no account of the form which it assumes.

Deville has furnished another interesting fact with respect to osmium, by determining the density of the vapour of osmic acid, which he has found $= 8\cdot 88$. This, if we take the generally received atomic weight for osmium, gives the atomic volume $\dfrac{131\cdot 6}{8\cdot 88} = 14\cdot 82$, indicating a condensation to 2 vols. If we now calculate back to the theoretical atomic weight, we get $(14\cdot 57 \times 8\cdot 88) - 32 = 97\cdot 38$, a number closely approaching 97, which, as we have seen, brings the equivalent of osmium into simple and harmonious relation with those of the other elements of the arsenic group.

The specific gravity of fused metallic osmium having been lately determined by Deville $= 21\cdot 4$, there can be little doubt that all the metals of the platinum family possess the same atomic volume when in the free state, about $4\cdot 6$ or $4\cdot 7$: the specific gravity of ruthenium is not yet known with accuracy, but such experiments as have been made render it improbable that it will prove an exception. This number is about one-fourth the mean of the atomic volumes of the long-recognized members of the arsenic group; but these latter differ so widely among themselves*, that the comparison is of little or no value. It would be desirable to get a good determination of the density of osmic acid in the solid state, so that its atomic volume might be calculated and compared with that of antimonious acid.

The specific heat of osmium, so far as its value as a physical character goes, *opposes* the introduction of this element into the arsenic group. It has been determined by Regnault $= \cdot 03063$; multiplying now by the equivalent 97, we have the product $2\cdot 9711$, thus placing osmium in the list of the elements (including the majority), for which the product of specific heat by

* Phosphorus .... $\dfrac{31}{1\cdot 83}$ (Schrötter) $= 16\cdot 94$.

  Arsenic .... $\dfrac{75}{5\cdot 67}$ (Herapath) $= 13\cdot 23$.

  Antimony .... $\dfrac{119}{6\cdot 7}$ (Karsten) $= 17\cdot 76$.

  Bismuth .... $\dfrac{207}{9\cdot 8}$ (Marchand & Scheerer) $= 21\cdot 12$.

atomic weight is nearly 3; while for phosphorus, arsenic, antimony, and bismuth, the product thus obtained is twice as great, or about 6. In this respect, however, osmium probably resembles nitrogen—the latter examined, as it necessarily is, in the gaseous form.

It is to be hoped that the conducting power for heat and electricity of compact osmium will soon be examined; nothing is as yet known of these characters.

Lastly, as regards the magnetic relations of the element : it is placed, with some doubt, by Faraday in the paramagnetic class; the metal and its protoxide were found to act feebly in this sense, while pure osmic acid is said to have shown itself clearly *diamagnetic*. The strongly diamagnetic character of phosphorus, antimony, and bismuth would render a re-examination of this point interesting. Arsenic, however, is said to be very feebly diamagnetic, and is placed by Faraday close to osmium in the list of metals examined, though on the opposite side of the line of magnetic neutrality or indifference.

Reviewing, now, the united physical and chemical characters of osmium, and comparing them with those of the generally recognized members of the " arsenic group," we are, I think, justified in concluding that here this curious metal should be placed in a natural arrangement of the elements; while important distinctions seem to separate it from some, at least, of the platinum metals, with which it is usually associated and described.

XLI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 235.]

Nov. 17, 1859.—Sir Benjamin C. Brodie, Bart., Pres., in the Chair.

THE following communications were read :—

" Researches on the Phosphorus-Bases."—No. VI. Phospham-monium-Compounds. By A. W. Hofmann, LL.D., F.R.S. &c.

In several previous communications I have shown that dibromide of ethylene is capable of fixing either one or two molecules of triethylphosphine, a monatomic and a diatomic bromide being formed, which I have respectively represented by the formulæ—

Monatomic bromide.

$$C_{18} H_{19} PBr_2 = (C_4 H_4)'' Br_2 + (C_4 H_5)_3 P = \begin{bmatrix} C_4 H_5 \\ C_4 H_5 \\ C_4 H_5 \\ (C_4 H_4 Br) \end{bmatrix} P \end{bmatrix} Br$$

and

Diatomic bromide.

$$C_{23} H_{34} P_2 Br_2 = (C_4 H_4)'' Br_2 + 2 ((C_4 H_5)_3 P) = \begin{bmatrix} (C_4 H_5)_2 \\ (C_4 H_5)_2 \\ (C_4 H_5)_2 \\ (C_4 H_4)'' \end{bmatrix}'' P_2 \end{bmatrix} Br_2$$
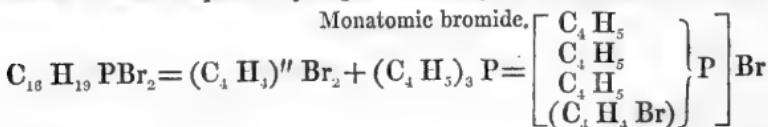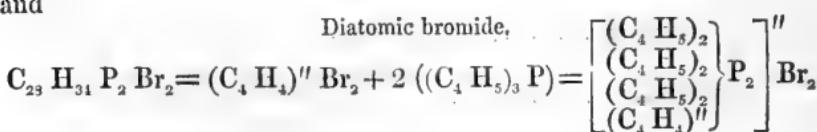
Ther are other products formed, resulting from secondary reactions.

It was not quite easy to obtain a sufficiently satisfactory experimental foundation for the diatomic nature of the second compound. This substance presents an extraordinary degree of stability; in its general characters it is closely allied to the numerous monatomic bromides, both of the nitrogen- and of the phosphorus-series, which in the course of these researches have come under my consideration. Lastly, the oxygenated derivative of the bromide resembles so perfectly the monammonium- and the monophosphonium-bases, that more than once during my experiments I was inclined to doubt the correctness of my interpretation.

There is no direct proof of the diatomic character of the compound. Why should we reject the simple formula deducible from experiment? The hydrocarbons $C_n H_n$ are very prone to molecular transformations without change of composition. The idea suggested itself, that the diatomic saline molecule might be split into two monatomic saline molecules,

$$[(C_4 H_5)_6 (C_4 H_1)'' P_2]'' Br_2 = 2\big([(C_4 H_5)_3 (C_2 H_2)' P] Br\big).$$
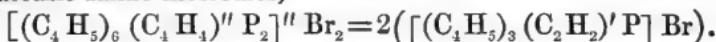
It is true $C_2 H_2$ figures in this formula as monatomic, whilst we should expect it endowed with diatomic substitution-power. But the connexion between composition and substitution-power is by no means finally settled; in fact, we know of many cases in which, under conditions not sufficiently established, the atomicity of a molecule changes: witness the radical "allyle," which is capable of replacing one or three equivalents of hydrogen.

But without going this length, the scission of diatomic ethylene into two monatomic molecules may take place in many other ways. The transformation of dibromide of ethylene into hydrobromic acid and bromide of vinyle,

$$(C_4 H_4)'' Br_2 = HBr + (C_4 H_3) Br,$$

is a familiar example. The splitting of the ethylene-compound into bromide of formyle and bromide of methyle,

$$(C_4 H_4)'' Br_2 = (C_2 H) Br + (C_2 H_3) Br,$$
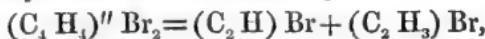
has never been observed, but did not appear altogether unlikely.
Our analytical methods are insufficient to distinguish between

$$[(C_4 H_5)_3 (C_2 H_2) P] Br \quad \text{and} \quad [(C_4 H_5)_3 (C_2 H) P] Br;$$
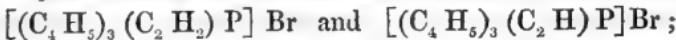
and what I have represented as a diatomic ethylene-compound might have been, after all, a monatomic bromide—the bromide of formyl-triethylphosphonium, the complementary methyle-compound

$$[(C_4 H_5)_3 (C_2 H_3) P] Br$$

existing possibly among the secondary products of decomposition.

In the presence of these and several similar self-raised objections, by which every observer endeavours to test the truth of his conclusions, I was induced again to appeal to experiment.

The prosecution of this line of the inquiry has led me to the discovery of a new class of diatomic bodies, which, while it confirms incontestably the correctness of my interpretation, appears to claim the attention of chemists for several other reasons.

I have established, in the first place, that the monatomic bromide

$$[(C_4 H_5)_3 (C_4 H_4 Br) P] Br$$

may be readily converted into the diatomic bromide

$$[(C_4 H_5)_6 (C_4 H_4)'' P_2]'' Br_2$$

by the simple addition of triethylphosphine. Nothing is easier than to prove the transformation, the platinum-salt of the two bases presenting a remarkable difference of solubility, and other differences not less striking.

To remove every doubt, the bromide, obtained by treatment of the brominated bromide with triethylphosphine, was converted into the corresponding iodide, which in its properties and composition was found to be identical in every respect with the characteristic iodide, which I have fully described in my last note upon this subject.

The transformation of the monatomic into what I have represented as the diatomic compound being satisfactorily established, the conclusive experimental demonstration of the diatomic nature of the latter presented itself without difficulty in the conception of bromides containing at once phosphorus and nitrogen, the molecular expression of which would no longer admit of division.

This class of dibromides actually exists ; they are readily produced by submitting the bromide of the brominated body to the action of ammonia or monamines instead of triethylphosphine.

I have formed as yet only three representatives of this new class of bodies, which I propose to designate as phosphammonium-compounds ; their examination is sufficient to fix the character of the class ; it would have been easy to construct scores of similar bodies.

Action of Ammonia upon the bromide of the brominated body.

The two substances, especially when in alcoholic solution, unite with evolution of heat—

$$[(C_4 H_5)_3 (C_4 H_4 Br) P] Br + H_3 N = \left[\begin{matrix} (C_4 H_5)_3 \\ (C_4 H_4)'' \\ H_3 \end{matrix}\right\} \begin{matrix} P \\ \\ N \end{matrix}\right]'' Br_2.$$
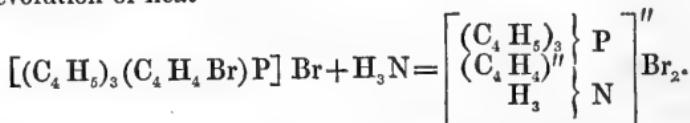
Both the bromide and the corresponding chloride are very soluble, and little adapted for analysis ; I have therefore fixed the nature of this body by the preparation and analysis of the platinum-compound. For this purpose the bromide generated in the above reaction was treated with oxide of silver ; it is thus converted into a powerfully alkaline solution obviously of the dioxide,

$$\left[\begin{matrix} (C_4 H_5)_3 \\ (C_4 H_4)'' \\ H_3 \\ \\ H_2 \end{matrix}\right\} \begin{matrix} P \\ \\ N \end{matrix}\right]'' \right\} O_4,$$
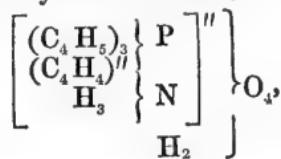
which, saturated with hydrochloric acid and mixed with dichloride of platinum, furnished a light-yellow crystalline platinum-salt, recrystallizable from boiling-water, and containing

$$[(C_4 H_5)_3 H_3 (C_4 H_4)'' PN]'' Cl_2, 2PtCl_2.$$
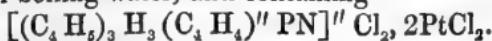
Action of Ethylamine and Trimethylamine upon the bromide of the brominated body.

The phenomena observed with ethylamine and trimethylamine are perfectly analogous. These substances furnish, with the brominated bromide, new and very soluble dibromides, containing respectively

$$\left[\begin{matrix} (C_4 H_5)_3 \\ (C_4 H_4)'' \\ (C_4 H_5) H_2 \end{matrix} \right\} \begin{matrix} P \\ N \end{matrix} \right]'' Br_2 \quad \text{and} \quad \left[\begin{matrix} (C_4 H_5)_3 \\ (C_4 H_4)'' \\ (C_2 H_3)_3 \end{matrix} \right\} \begin{matrix} P \\ N \end{matrix} \right]'' Br_2,$$
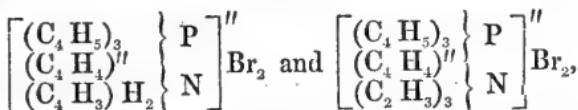
which, by treatment with oxide of silver, are converted into the corresponding powerfully alkaline oxides

$$\left[\begin{matrix} (C_4 H_5)_3 \\ (C_4 H_4)'' \\ (C_4 H_5) H_2 \\ \\ H_2 \end{matrix} \right\} \begin{matrix} P \\ N \\ \\ \end{matrix} \right]'' O_4 \quad \text{and} \quad \left[\begin{matrix} (C_4 H_5)_3 \\ (C_4 H_4)'' \\ (C_2 H_3)_2 \\ \\ H_2 \end{matrix} \right\} \begin{matrix} P \\ N \\ \\ \end{matrix} \right]'' O_4,$$
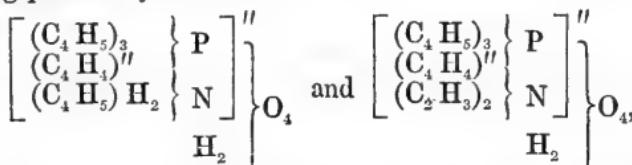
and yield, by saturation with hydrochloric acid and precipitation with dichloride of platinum, two splendid platinum-salts crystallizing in long golden-yellow needles, and containing respectively

$$[(C_4 H_5)_4 (C_4 H_4)'' H_2 PN]'' Cl_2, 2Pt Cl_2, \quad \text{and}$$
$$[(C_4 H_5)_3 (C_2 H_3)_3 (C_4 H_4)'' PN]'' Cl_2, 2Pt Cl_2.$$
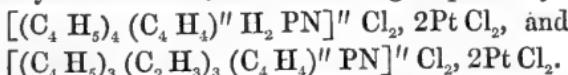
By the formation of the phosphammonium-compounds, the nature both of the diammonium- and of the diphosphonium-series appears to me finally established.

It will be interesting to ascertain whether the brominated bromide, when submitted to the action of monarsines and monostibines, will give rise to the formation of phospharsonium- and phospho-stibonium-bases. The solution of this question will not be difficult.

"On the Behaviour of the Aldehydes with Acids." By A. Geuther, Esq., and R. Cartmell, Esq.

The authors of this paper, with a view of obtaining a series of combinations homologous with those already obtained from glycol by Wurtz—viz. diacetate of glycol, $\left. \begin{matrix} C_4 H_4 \\ C_4 H_3 O_2 \\ C_4 H_3 O_2 \end{matrix} \right\} O_4$, and the isomeric body of Geuther from common aldehyde, by the action of anhydrous acetic acid,—have subjected common aldehyde, acroleine, and oil of bitter almonds to the action of hydrochloric, hydriodic, and sulphurous acids.

I. *Acroleine,—Metacroleine.*

1. *Acroleine and Hydrochloric Acid.*

By acting on acroleine, $C_6 H_4 O_2$, with dry hydrochloric acid gas, a body is formed of the composition $C_6 H_5 O_2 Cl$, resulting from a direct combination of one atom of aldehyde with one atom of the acid. This substance is insoluble in water, and can be washed with it in order to free it from any excess of acid or acroleine which may be still present. By drying, which can only be done over sulphuric acid at low temperatures, the body, for which the authors propose

the name of hydrochlorate of acroleine, is obtained in a mass of white crystals, presenting a texture like that of velvet. It melts at 32° C. into a thick oil, having a smell of slightly rancid fat. It is readily soluble in alcohol or ether, on evaporation of which it remains behind as a thick oil. When boiled with water, it remains, as far as can be seen, unchanged. Dilute solutions of the alkalies appear not to act on it. Heated with solution of ammonia in a sealed tube at 100° C., it is decomposed, chloride of ammonium and acroleine ammonia being the result. It does not combine with bichloride of platinum when in solution in alcohol, and very slowly reduces boiling ammoniacal solution of nitrate of silver. Heated alone, it decomposes into acroleine and hydrochloric acid. By the action of concentrated hydrochloric acid acroleine is set free. Dilute sulphuric and nitric acids decompose it likewise, setting acroleine free. Heated with hydrate of potash it gives off hydrogen; and there distils at the same time an oily body, which solidifies into magnificent colourless crystals, analyses of which prove it to be an isomeric acroleine, for which the authors propose the name *Metacroleine*.

Metacroleine as thus obtained is insoluble in water, but is capable of being recrystallized from alcohol or ether. The crystals form very long needles, more especially when melted metacroleine before solidifying is allowed to flow about in a glass tube. They resemble very much in appearance crystals of acetamide, possess a peculiar aromatic smell, and have a taste at first producing a cooling and afterwards a burning sensation. They are lighter than water. They melt at about 50° C., becoming solid at about 45°C. Before melting they are somewhat volatilizable, on which account they can be distilled in the vapour of water. On being heated, metacroleine is changed into common acroleine. Dilute alkalies do not effect any change in this substance. By heating with mineral acids, common acroleine is set free. On leading dry hydrochloric acid gas over metacroleine in a bulb-tube, the metacroleine melts and combines with the acid, producing the already-named hydrochlorate of acroleine. From this behaviour, the authors believe the acroleine contained in the compound of hydrochloric acid to be metacroleine, and not common acroleine. If metacroleine be viewed as $C_{12} H_8 O_4$, the formula of the hydrochloric acid compound would then be $C_{12} H_8 O_4$, 2HCl; and the formation of metacroleine may be assumed to take place according to the following equation, $C_{12} H_8 O_4$, $2HCl + 2KOHO = C_{12} H_8 O_4 + 2KCl + 4HO$. The evolution of hydrogen has been found to be the result of a secondary action.

2. *Acroleine and Hydriodic Acid.*

These substances act very violently on each other if the acid in the gaseous form be led into acroleine, producing a hissing noise, as when red-hot iron is plunged into water. The resulting substance is insoluble in alcohol, ether, acids, and alkalies. Bisulphide of carbon dissolves out a little free iodine. Heated alone, iodine is set free.

3. *Acroleine and Water.*

Acroleine mixed with two or three times its volume of water, and exposed to the temperature of boiling water for eight days, under-

goes a gradual change. Acrylic acid is produced, and a resinous substance, soluble in ether, melting at about 60°, and becoming solid at 55°C. At common temperatures it is hard and brittle, like resin. The per-centage composition of this resin, on analysis, was found to be the same as that obtained by Redtenbacher, and named Disacrylic resin*, viz. carbon 66·6, hydrogen 7·4.

4. *Metacroleine and Hydriodic Acid.*

When dry hydriodic acid gas is passed over dry metacroleine, the latter melts, and changes into a heavy yellow solution, resembling in smell and appearance the hydrochlorate of acroleine. It can be washed with water, and appears at ordinary temperatures to solidify into crystals; placed over sulphuric acid to dry, it decomposes, becoming brown, and setting iodine free. From the analogy in its formation, this compound can be properly viewed as hydriodate of acroleine.

II. *Aldehyde.*

1. *Aldehyde and Hydrochloric Acid.*

Lieben found that by the action of hydrochloric acid on aldehyde, a body of the composition $C_3 H_3 O_2 Cl_2$ was produced, having a constant boiling-point of from 116° to 117° C.†

The authors confirm Lieben's statement as to the replacement of O_2 by Cl_2 in two atoms of aldehyde, and have further obtained a new combination, analysis of it giving the formula as $C_{12} H_{12} O_4 Cl_2$, in which two equivalents of oxygen are replaced by the same number of equivalents of chlorine in three atoms of aldehyde. By the action of water, this compound, like that of Lieben, is resolved into hydrochloric acid and aldehyde. By heat, it is broken up into aldehyde and the body $C_3 H_3 O_2 Cl_2$. The authors propose for it the name protoxychloride of aldehyde.

2. *Aldehyde and Hydriodic Acid.*

By the action of hydriodic acid on aldehyde a compound is produced that decomposes with water into the aldehyde and the acid again, on which account it could not be purified. On heating, it is suddenly decomposed at 70° C., leaving a black resinous residue, which on distillation gave off vapours of iodine. In its mode of formation it is analogous to the bodies produced by the action of hydrochloric acid on aldehyde.

3. *Aldehyde and Sulphurous Acid—Elaldehyde.*

Dry sulphurous acid gas led into anhydrous aldehyde in cold water is absorbed with great avidity, 11 grammes of aldehyde absorbing 19 grammes of the acid, whilst an increase of volume takes place. The absorption-coefficient of aldehyde for this acid was found to be 1·4 times greater than that of alcohol for the same, and seven times greater than that of water for it. No chemical combination appears to take place, as, on passing a stream of carbonic acid through the fluid at a slightly elevated temperature, almost all the sulphurous acid can be driven out again. If aldehyde, saturated with sulphurous acid, be left for about a week at ordinary temperatures in a well-stoppered

* Chem. Gaz. vol. i. p. 744. † Ibid. vol. xvi. p. 215.

Y 2

bottle, it suffers in this time almost a complete change into a body for which the authors propose at present the name Elaldehyde. To obtain it pure, the fluid is mixed with as much water as is necessary to dissolve it up; the acid is saturated by degrees with chalk, and the fluid obtained is distilled so long as oily drops pass into the receiver. The common aldehyde is separated in a resinous form by digesting for some time with solution of caustic soda or potash. By repeated distillation, the elaldehyde can be obtained free from everything but a little water. Analysis gives the formula of this aldehyde as $C_4 H_4 O_2$. It is therefore isomeric with common aldehyde. As it was obtained in quantity by the foregoing method, its properties were further examined. Its boiling-point was found to be 124° C., and solidifying-point 10° C. Whilst solidifying it likewise starts into crystals, the melting-point of which is also 10° C. The aldehyde here described under the name Elaldehyde is identical with that of Weidenbush*. Its mode of production from common aldehyde is the same; its boiling-point likewise agrees with that of the aldehyde of Weidenbush.

The elaldehyde of Fehling the authors believe to be identical with that they have obtained, and also that obtained by Weidenbush. That which goes far to prove the identity of the two latter is their vapour-densities. That of Weidenbush's is given as 4·58, whilst that of Fehling's is 4·52; both are converted into common aldehyde by heating gently with dilute sulphuric acid, and both crystallize at low temperatures. The only material discrepancy between them is the boiling-point of 94° C. given by Fehling for elaldehyde, whilst Weidenbush gives the boiling-point of his aldehyde as 125° C.

III. *Oil of Bitter Almonds.*

1. *Oil of Bitter Almonds and Hydrochloric Acid.*

This acid does not combine with oil of bitter almonds. Experiments made in sealed tubes, heated first to 100° C., and afterwards to 200°, gave no signs of a combination having been effected.

2. *Oil of Bitter Almonds and Hydriodic Acid.*

Much better results can be obtained when hydriodic acid is allowed to act on oil of bitter almonds. The gas is absorbed, producing an increase of volume and of temperature, and at the same time a little water. At the end of the operation two layers appear, of a dark-brown colour. The upper one, which is about a sixth part of the quantity of the under one, consists of concentrated hydriodic acid, whilst the under one, a heavy oil, is a compound of iodine and oil of bitter almonds. To obtain the substance in a pure state, it was first washed well with water to remove excess of the acid; next treated with moderately strong solution of sulphite of soda, to remove any excess of oil; lastly, on washing with water, the salt was removed from it. It can be dried rapidly over sulphuric acid at a temperature not higher than 20° C. A higher temperature produces gradual decomposition. In the preparation of this substance, 6 grammes of oil of bitter almonds absorbed 11 grammes of hydriodic acid gas. Analyses of the substance lead to the formula

* Chem. Gaz. vol. vii. p. 34.

$C_{42}H_{18}O_2I_4$, which will be observed to be 3 atoms of oil of bitter almonds, in which $2(O_2)$ is replaced by $2(I_2)$. The authors propose for it the name Oxyiodide of Benzaldehyde. The substance thus obtained melts at 28° C., and solidifies at about 25° C. into almost colourless rhombic plates if rapidly cooled down. When in a liquid state, the crystals mostly occur in groups of long needles. The colour of the substance in a melted state is brownish yellow; at moderate temperatures, and on standing in the air, it becomes still darker in colour. It possesses a smell very much resembling cress. It volatilizes at common temperatures, its vapour attacking the eyes powerfully. Its vapour at higher temperatures, when carried away by that of water, becomes more and more intolerable, producing a very inflammatory effect on the eyes and nose, which is more painful and permanent than that from acroleine. It is insoluble and sinks in water, but can be distilled in the vapour of it. Watery solutions of carbonates and sulphites of the alkalies do not act on it. Alcoholic solution of potash decomposes it by degrees on heating a little, producing much iodide of potassium, some benzoic acid, and an oily body that remains dissolved in the alcohol, which is not oil of bitter almonds. Alcoholic and watery solutions of ammonia change it slowly into iodide of ammonium and oil of bitter almonds. Boiled with solution of nitrate of silver, it yields iodide of silver, and a smell of oil of bitter almonds. Concentrated hydrochloric acid changes it by degrees, becoming brown; concentrated sulphuric acid dissolves it on heating, with the separation of iodine.

In conclusion, the authors remark that the action of hydrochloric acid on aldehyde may be regarded as consisting in the replacement of two equivalents of oxygen by two of chlorine in one, two, or three atoms of this body : thus, Aldehyde containing chlorine.

1 atom of aldehyde	$C_4H_4O_2$	$C_4H_4Cl_2$		
2 ,, ,,	$C_8H_8O_4$	$C_8H_8O_2Cl_2$	Lieben's body.	
3 ,, ,,	$C_{12}H_{12}O_6$	$C_{12}H_{12}O_4Cl_2$	{ Protoxychloride of aldehyde.	

The action of hydriodic acid on oil of bitter almonds gives rise also to a body derived from 3 atoms of this aldehyde, in which $2(O_2)$ is replaced by $2(I_2)$.

3 atoms of oil of bitter almonds,   Oxyiodide of Benzaldehyde,

$$C_{42}H_{18}O_6 \qquad\qquad C_{42}H_{18}O_2I_4.$$
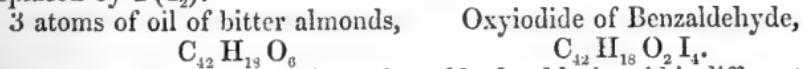
In the case of acroleine, the action of hydrochloric acid is different; it combines directly with it, no elimination of water taking place. If we conceive, however, that, in the action of this acid on common aldehyde, the water which is there produced is the effect of a further decomposition, then we may readily suppose that, if this further decomposition had taken place in the case of hydrochloric acid and acroleine, a body derived from two atoms of acroleine, and having O_2 replaced by Cl_2, corresponding to the second term in the combination of aldehyde and chlorine, would have been the result; thus—

2 atoms of hydrochlorate of acroleine—

$$C_{12}H_{10}O_4Cl_2 - 2(HO) = C_{12}H_8O_2Cl_2,$$ corresponding to the term $C_8H_8O_2Cl_2$ in common aldehyde.
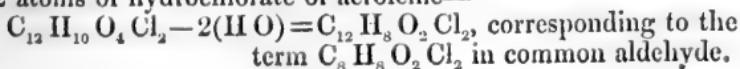
There is a curious connexion which may be mentioned, in this substitution of chlorine for oxygen in aldehyde, between the formula of these bodies containing chlorine, and those of the isomeric modifications of aldehyde.

"Experiments on some of the Various Circumstances influencing Cutaneous Absorption." By Augustus Waller, M.D., F.R.S.

"On Spontaneous Evaporation." By Benjamin Guy Babington, M.D., F.R.S. &c.

The object of this communication is to make known certain powers of attraction and repulsion, hitherto, as far as I know, unnoticed, which are possessed by soluble substances in relation to their solvent, and which, in the case of water (the solvent here considered), are measured by the amount of loss, on spontaneous evaporation, in the weight of solutions of different salts and other substances, as compared with the loss of weight in water.

The force which holds together the particles of a vaporizable liquid is gradually overcome, if that liquid be exposed to air, by another force which separates, expands, and diffuses those particles in the form of vapour; and this separation takes place, even at a common temperature, so rapidly, provided the surface be sufficiently extensive, that an easy opportunity is afforded of determining the loss of weight by a common balance.

A subject for investigation, possessing much interest, thus presents itself; and in its pursuit some new and unexpected results are encountered.

The method which I have pursued has been to expose to the atmosphere, for a definite period, solutions of different salts, and also pure water under like conditions of quantity and area, temperature, atmospheric moisture, and atmospheric pressure.

Different salts and other soluble substances are thus found to possess, when in solution, different powers of retarding or accelerating evaporation, and hence, from its amount, as compared with that which takes place in pure water, we can estimate the comparative value of those powers.

The powers themselves being established as facts, the next point is to endeavour to discover the cause or causes on which they depend; and a wide field of inquiry is thus opened.

The following are the instruments which have been employed:—

1. A balance, for one of the scales of which is substituted a flat metal plate, six inches square, on which the vessels to be weighed can be conveniently supported. This balance will turn sensibly at a grain, even with a weight of 4 lbs. on either side.

2. A number of copper pans tinned within, all of the same size, being precisely 5 inches square inside, with perpendicular sides $\frac{3}{4}$ths of an inch in height, also a number of earthenware pans of the same dimensions, The area of 25 square inches has been chosen, partly because this size is convenient for manipulation, and partly because the results obtained can be easily represented in decimals. This facility of decimal calculation would be of importance should such pans come into general use as hygrometers, for which purpose they are well adapted.

3. Specific gravity bottles and counterpoises.

4. Thermometers of various degrees of delicacy and range, for ascertaining freezing, temperate, and boiling points.

5. Test tubes for use, in connexion with these thermometers, as well in freezing mixtures as over the spirit lamp.

6. A barometer.

7. Various salts and other soluble substances, furnishing, when in solution, the materials for examination.

The mode of procedure which I have adopted has been, to state my facts in the form of propositions, and to prove each of these propositions by experiments.

The propositions are as follows :—

1st proposition.—That in many aqueous solutions of salts and other soluble substances evaporation is retarded, as compared with the evaporation of water.

2nd proposition.—That in solutions of salts which retard evaporation, that retardation is in proportion to the quantity of the salt held in solution.

3rd proposition.—That different salts and other substances soluble in water have different degrees of power in retarding its evaporation.

4th proposition.—That the power of retarding evaporation does not depend on the specific gravity of a solution.

5th proposition.—That in aqueous solutions of salts, the power of retardation does not depend on the base, whether we compare solutions containing like weights of the salt, or solutions of like specific gravities.

6th proposition.—That in aqueous solutions of salts, the power of retarding evaporation does appear to depend upon the salt radical or acid, although the retardation is not altogether independent of the influence of the base.

7th proposition.—That salts with two equivalents of an acid have a greater power of retarding evaporation than salts with one equivalent. There are, however, exceptions.

8th proposition.—That there are some salts which, being dissolved in water, do not retard its evaporation, and some salts which, so far from retarding, actually accelerate evaporation.

The truth or probability of the foregoing propositions is established by numerous experiments, but in this abstract I shall, for the sake of brevity, only state the result of one or two experiments in proof of each.

The first proposition is proved by the fact that a solution of hydrochlorate of soda in the proportion of 480 grains to four measured ounces of water, when exposed under the conditions already stated to spontaneous evaporation, lost only 33 grains in weight after twelve hours' exposure—while four ounces by measure of water lost 53 grains, —and after twelve hours' further exposure lost only 109 grains, while the water lost 174 grains ; that is, the water, as compared with the solution, lost weight in the ratio nearly of 5 to 3.

The second proposition is proved by the fact that a solution of 240 grains of hydrochlorate of soda in four ounces by measure of water lost in twelve hours 73 grains by evaporation, while four ounces by measure of pure water lost 81 grains,—this is in a proportion of only about 8 of the latter to 7 of the former ; whereas, when double

the quantity or 480 grains of salt were dissolved, the pure water, as compared with the solution, lost in the proportion of 5 to 3.

The third proposition is proved by the fact that a solution of 480 grains of nitrate of potassa in 4 ounces or 1920 grains of water lost in twelve hours 95 grains; while a solution of the same strength of hydrochlorate of soda lost only 70 grains; and again, a solution of loaf-sugar, in which 480 grains were dissolved in 1920 grains of water, lost in 20 hours 175 grains, while a like solution of hydrochlorate of soda lost only 117 grains.

The fourth proposition is proved by the fact that 480 grains of gum-arabic dissolved in 1920 of water had a specific gravity of 1·072, while a solution of hydrochlorate of soda of like strength had a specific gravity of 1·149; after 11¼ hours, the former had lost by evaporation 71 grains, while the latter had lost only 50 grains. Here, therefore, the solution of the lighter specific gravity was *less* retarded in its evaporation than the heavier solution. In contrast with this fact, a solution of hydrochlorate of ammonia of 480 grains to 1920 grains of water, having a specific gravity of only 1·060, lost by evaporation, in 8 hours and 44 minutes, 17 grains, while a like solution of hydrochlorate of soda lost 24 grains. Here, then, the solution of lighter specific gravity was *more* retarded in its evaporation than the heavier solution. The conclusion is decisive that specific gravity has no necessary connexion with the phænomena.

The fifth proposition is proved by the fact that in the following solutions of salts of potassa, all of the same strength (namely 1 salt to 10 water), a difference in the amount of evaporation in each will be observed to have taken place; and it must be borne in mind that in solutions so weak we cannot expect that difference to be very great.

The reason for employing weak solutions was the necessity for having all of the same strength, one in ten being the extent to which the least soluble salt submitted to examination, namely, the sulphate of potassa, will, at a low temperature, dissolve. grains.

	grains.
Acetate of potassa lost in 35 hours	145
Bicarbonate of potassa lost in 35 hours	131
Carbonate of potassa lost in 35 hours	115
Ferro-cyanate of potassa lost in 35 hours	110
Hydrochlorate of potassa lost in 35 hours	98
Nitrate of potassa lost in 35 hours	117
Sulphate of potassa lost in 35 hours	132
Tartrate of potassa lost in 35 hours	151

The above solutions were next made all of one specific gravity, namely 1·060, temp. 62° Fahr., instead of being all of one strength, and the following is the result:—

	grains.
Acetate of potassa lost in 16½ hours	46
Bicarbonate of potassa lost in 16½ hours	45
Carbonate of potassa lost in 16½ hours	35
Ferro-cyanate of potassa lost in 16½ hours	41
Hydrochlorate of potassa lost in 16½ hours	32
Nitrate of potassa lost in 16½ hours	39
Sulphate of potassa lost in 16½ hours	42
Tartrate of potassa lost in 16½ hours	43

The sixth proposition is rendered probable by the following experiment, in which solutions are employed of acetic, nitric, sulphuric, and hydrochloric acids, combined respectively with potassa, soda, and ammonia, in the proportion of 100 grains of the salt to 1000 grains of water. After the expiration of 10 hours and 20 minutes, the solution of the three acetates lost respectively, for the potassa salt 35 grs., for the soda salt 35 grs., and for the ammonia salt 28 grs. In the solutions of the three nitrates, the loss was respectively 24, 25, and 25. In the solutions of the three sulphates, the loss was 30 grs., 37 grs., and 29 grs. respectively, while in the solutions of the hydrochlorates it was 17, 18, and 19 grains.

The seventh proposition is proved by an experiment in which a solution of 100 grains of carbonate of potassa dissolved in 1000 grains of water is compared with a like solution of bicarbonate of potassa. In ten hours the solution of the carbonate lost 45 grains, while that of the bicarbonate lost only 36 grains. In comparing like proportions and quantities of sulphate and bisulphate of potassa, the respective losses in 13 hours were, for the former 53 grains, for the latter 45 grains. Similar comparisons of the acetate and binacetate of ammonia, phosphate and biphosphate, sulphate and bisulphate of potassa, tartrate and bitartrate of soda show like results. In the course of investigating this proposition it was remarked incidentally that in all the salts examined, with the single exception of carbonate and bicarbonate of soda, the bin-acid solution (the proportion by weight of salt to water being equal) is of less specific gravity than the mono-acid solution, though possessing a greater power of retarding evaporation.

The eighth proposition, which seems extraordinary and even paradoxical, is proved by an experiment in which *saturated* solutions of—1, ferro-cyanate of potassa, 2, bitartrate of potassa, 3, sulphate of copper, 4, chlorate of potassa, and 5, distilled water, were compared. In 9 hours and 20 minutes, their losses by evaporation were respectively 34 grs., 38 grs., 34 grs., 29 grs., and 29 grs., where we perceive that in the chlorate of potassa solution there has occurred no retardation at all, while in the following experiment, in which 120 grains of each of the salts examined were dissolved in 1200 grains of water, namely,—1, solution of sulphate of copper, 2, solution of ferro-cyanate of potassa, 3, solution of carbonate of soda, and 4, distilled water, the number of grains lost by evaporation after 15½ hours' exposure were,—1, 120 grs.; 2, 113 grs.; 3, 106 grs.; 4, 103 grs.

It is thus perceived that in all the three solutions a more rapid evaporation had taken place than in distilled water alone.

One or two other propositions are in process of investigation.

The paper concludes with a table of the freezing-points, boiling-points, and specific gravities, as well of weak as of saturated solutions, of the salts which have been submitted to examination.

[Continued from p. 238.]

February 1, 1860.—Sir C. Lyell, Vice-President, in the Chair.

The following communications were read:—

1. " On some Cretaceous Rocks in Jamaica." By Lucas Barrett, Esq., F.G.S., Director of the Geological Survey in Jamaica.

On the north side of Plantain-Garden River, three miles west of Bath, shale and limestone overlie conglomerate. The limestone contains *Inoceramus, Hippurites,* and *Nerinæa.* Higher up the river similar fossiliferous limestone occurs in vertical bands, succeeded by conglomerates, which separate it from massive porphyries.

On the medial ridge of mountains, also, at an elevation of 2500 feet above the sea, Hippurite-limestone, with black flints containing *Ventriculites,* rests on porphyry and hornblende-rock. These igneous rocks are interstratified with shales and conglomerates.

2. " On the Occurrence of a mass of Coal in the Chalk of Kent." By R. Godwin-Austen, Esq., F.G.S.

This piece of coal was met with in cutting the tunnel on the Chatham and Dover Railway, between Lydden Hill and Shepherds-well. It weighed about 4 cwts., and was 4 feet square, with a thickness of 4 inches at one part, increasing to 10 inches at another. It was imbedded in the chalk, where the latter was free from faults. The coal is friable, highly bituminous, and burns readily, with a peculiar smell, like that of retino-asphalt. It resembles some of the Wealden or Jurassic coals, and is unlike the true coal of the coal-measures. Mr. Godwin-Austen stated his belief that during the Cretaceous period some beds of lignite or coal of the preceding Jurassic period lay near the sea-margin, or along some river, so as to be covered by water; and hence portions could be lifted off by ice, and so drifted away (like the granitic boulder found in the Chalk at Croydon) until the ice was no longer able to support its load.

3. " On some Fossils from the Grey Chalk near Guildford." By R. Godwin-Austen, Esq., F.G.S.

In the cast of the body-chamber of a large *Nautilus elegans,* from the Grey Chalk of the Surrey Hills, near Guildford, the author found (the specimen having been broken up by frost) some lumps of iron-pyrites, and numerous specimens of *Aporrhais Parkinsoni,* with fragments of *Turrilites tuberculatus, Ammonites Coupei, A. varians,* and *Inoceramus concentricus.* These species are either rare in the Grey Chalk or not known to the author as occurring in this bed; and he believes that the specimens referred to were accumulated in the shell of the Nautilus (possibly by the animal having taken them as a meal shortly before death) at a different zone of sea-depth to that in which the Nautilus and its contents sank and became fossilized. Mr. Godwin-Austen referred to these specimens as being indicative of the contemporary formation of different deposits with their peculiar fossils, at different sea-zones; of the transport of the inhabitants of one zone to the deposits of another; and as a possible explanation of the abundance of small angular fragments of Mollusks, Echinoderms, and Crustaceans, in the midst of the very finest Cretaceous sediment.

4. "On the Probable Events which succeeded the Close of the Cretaceous Period." By S. V. Wood, jun., Esq.

The object of this paper was to show that the close of the Secondary period was followed by the formation of a continent having a great extent from east to west, and at that time chiefly occupying low latitudes; that this direction of continent prevailed throughout the Tertiary period; and that in certain portions of the southern hemisphere, particularly in Australia and New Zealand, there have been preserved portions of the Secondary continent with isolated remnants of the Secondary Mammalia and Gigantic Birds. These conclusions were arrived at by a consideration of the direction of the principal volcanic axes in the Secondary and Tertiary periods. The Secondary continent was (the author considered) mainly influenced in the northern hemisphere by volcanic axes which came into action at the close of the Carboniferous, and continued through the Secondary Period. These axes were that of the Oural, that of the north of England prolonged into Portugal, and that of the Alleghanies, having all a north and south direction, supervening upon volcanic axes having a direction at right angles to them, which had prevailed during the Newer Palæozoic period. From this circumstance an inference was drawn that the Secondary continents had generally a trend from north to south, governed by volcanic bands having this direction; while, as the Secondary formations indicate a great extent of sea over the northern hemisphere, the bulk of the Secondary continent lay in the southern hemisphere.

The elevation of the bed of the Cretaceous sea, it was inferred, was due to volcanic forces acting from east to west; and the author adduced evidence of this action having become perceptible during the later part of the Cretaceous period. He considered that the direction of all the Post-cretaceous lines of volcanic action governed the direction of the continent during the Post-cretaceous period, and pointed out that these were all in an easterly and westerly direction, coincident with the existing volcanic band which extends from the Azores to the Caspian, and thence (with an interval of intense earthquake-action between the Caspian and Bengal) extends to the Society Isles. He concluded that they gave rise to a continent extending from the Caribbean Sea to the Society Isles—many reasons uniting to show a land-connexion between America and Europe at the dawn of the Tertiary period, the submerged continent of Oceanica also indicating the easterly extension of Southern Asia; and that, since this continent receded to the north at the dawn of the Tertiary period before the inroad of the Nummulitic Sea (which stretched from the south-east through Western Asia and Southern Europe, and was, as the author conceives, the oceanic equivalent of the Eocene basins of Europe), the greater portion of the deposits formed in the interval between Cretaceous and Eocene times must be now under the Southern Oceans.

The author then adverted to the circumstance that the recent great wingless Birds and the nearest living affinities of all the Secondary Mammalia yet known occur only in the Southern hemisphere. From this, and from some considerations as to the

Vegetation, he concluded that, while parts of the Secondary conti-
nent yet remain in that hemisphere incorporated more or less into
the Post-cretaceous continent, other parts of it, such as Australia
and New Zealand, have remained isolated up to the present time to
an extent sufficient to preclude the migration of Mammalia and
wingless Birds. He inferred that the wingless Birds, excepting the
swift Struthionidæ, have been preserved solely by isolation from the
Carnivora, which do not appear as an important family until the
Pliocene age; and he instanced the *Gastrornis* of the Eocene (which
had affinities with the Solitaire and Notornis) as evidence that the
apterous birds had survived until that period.

An inference was then drawn that the remains of the Secondary
continent, accumulated to the southward, caused cold currents to
flow to the southern shores of the Post-cretaceous continent, causing
the extinction of the bottom-feeding and shore-following Tetra-
branchiata, to which Mr. Wood attributes the destruction of the
Cestracionts which fed on them, and that of the marine Saurians
that fed on the Cestracionts. The preservation of the Dibranchiata,
on the contrary, was attributed to their being ocean-rangers. The
extinction of the Megalosauria he attributed to the effect produced
on vegetation by the alternation of dry seasons during the year,
brought about by a great equatorial extent of land,—the extinction
of the herbivorous Megalosauria, by this cause, involving that of the
carnivorous.

The author also alluded to the contiguity of volcanos to the seas
or great waters, which he considered to admit of explanation by
every volcanic elevation causing a corresponding and contiguous
depression, which either brings the sea or collects the land-drainage
into contiguity with the volcanic region; and in conclusion he
alluded to the law of natural selection and correlation of growth
lately advanced by Mr. Darwin, in the soundness of which he
asserted his belief.

ROYAL INSTITUTION OF GREAT BRITAIN.

March 9, 1860.—" On Lighthouse Illumination—the Electric
Light." By Professor Faraday, D.C.L., F.R.S.

The use of light to guide the mariner as he approaches land, or
passes through intricate channels, has, with the advance of society
and its ever increasing interests, caused such a necessity for means
more and more perfect, as to tax to the utmost the powers both of
the philosopher and the practical man, in the development of the
principles concerned, and their efficient application. Formerly the
means were simple enough; and if the light of a lantern or torch
was not sufficient to point out a position, a fire had to be made in
their place. As the system became developed, it soon appeared
that power could be obtained, not merely by increasing the light,
but by directing the issuing rays : and this was in many cases a
more powerful and useful means than enlarging the combustion; leading
to the diminution of the volume of the former with, at the same time,
an increase in its intensity. Direction was obtained, either by the
use of lenses dependent altogether upon refraction, or of reflectors
dependent upon metallic reflexion. [And some ancient specimens

of both were shown.] In modern times the principle of total reflexion has also been employed, which involves the use of glass, and depends both upon refraction and reflexion. In all these appliances much light is lost: if metal be used for reflexion, a certain proportion is absorbed by the face of the metal; if glass be used for refraction, light is lost at all the surfaces where the ray passes between the air and the glass; and also in some degree by absorption in the body of the glass itself. There is, of course, no power of actually increasing the whole amount of light, by any optical arrangement associated with it.

The light which issues forth into space must have a certain amount of divergence. The divergence in the vertical direction must be enough to cover the sea from the horizon, to within a certain moderate distance from the shore, so that all ships within that distance may have a view of their luminous guide. If it have less, it may escape observation where it ought to be seen; if it have more, light is thrown away which ought to be directed within the useful degree of divergence: or if the horizontal divergence be considered, it may be necessary so to construct the optical apparatus, that the light within an angle of 60° or 45° shall be compressed into a beam diverging only 15°, that it may give in the distance a bright flash having a certain duration instead of a continuous light,— or into one diverging only 5° or 6°, which, though of far shorter duration, has greatly increased intensity and penetrating power in hazy weather. The amount of divergence depends in a large degree upon the bulk of the source of light, and cannot be made less than a certain amount, with a flame of a given size. If the flame of an Argand lamp, $\frac{7}{8}$ths of an inch wide and $1\frac{1}{2}$ inch high, be placed in the focus of an ordinary Trinity House parabolic reflector, it will supply a beam having about 15° divergence: if we wish to increase the effect of brightness, we cannot properly do it by enlarging the lamp flame; for though lamps are made for the dioptric arrangement of Fresnel, which have as many as four wicks, flames $3\frac{1}{2}$ inches wide, and burn like intense furnaces, yet if one be put into the lamp place of the reflector referred to, its effect would chiefly be to give a beam of wider divergence; and if to correct this, the reflector were made with a greater focal distance, then it must be altogether of a much larger size. The same general result occurs with the dioptric apparatus; and here, where the four-wicked lamps are used, they are placed at times nearly 40 inches distant from the lens, occasioning the necessity of a very large, though very fine, glass apparatus.

On the other hand, if the light could be compressed, the necessity for such large apparatus would cease, and it might be reduced from the size of a room to the size of a hat: and here it is that we seek in the electric spark, and such like concentrated sources of light, for aid in illumination. It is very true, that by adding lamp to lamp, each with its reflector, upon one face or direction, power can be gained; and in some of the revolving lights, ten lamps and reflectors unite to give the required flash. But then not more than three of these faces can be placed in the whole circle; and if a fixed light be required in all directions round the lighthouse, nothing better

has been yet established than the four-wicked Fresnel lamp in the centre of its dioptric and catadioptric apparatus. Now the electric light can be raised up easily to an equality with the oil lamp, and if then substituted for the latter, will give all the effect of the latter; or by expenditure of money it can be raised to a five or tenfold power, or more, and will then give five- or tenfold effect. This can be done, not merely without increase of the volume of the light, but whilst the light shall have a volume scarcely the 2000th part of that of the oil flame. Hence the extraordinary assistance we may expect to obtain by diminishing the size, and perfecting the optical part of the apparatus.

Many compressed intense lights have been submitted to the Trinity House; and that corporation has shown its great desire to advance all such objects and improve the lighting of the coast, by spending, upon various occasions, much money and much time for this end. It is manifest that the use of a lighthouse must be never failing, its service ever sure; and that the latter cannot be interfered with by the introduction of any plan, or proposition, or apparatus, which has not been developed to the fullest possible extent, as to the amount of light produced,—the expense of such light,—the wear and tear of the apparatus employed,—the steadiness of the light for 16 hours,—its liability to extinction,—the amount of necessary night care,—the number of attendants,—the nature of probable accidents,—its fitness for secluded places, and other contingent circumstances, which can as well be ascertained out of a lighthouse as in it. The electric spark which has been placed in the South Foreland High Light, by Prof. Holmes, to do duty for the six winter months, had to go through all this preparatory education before it could be allowed this practical trial. It is not obtained from frictional electricity, or from voltaic electricity, but from magnetic action. The first spark (and even magnetic electricity as a whole) was obtained twenty-eight years ago. (Faraday, Philosophical Transactions, 1832, p. 32.) If an iron core be surrounded by wire, and then moved in the right direction near the poles of a magnet, a current of electricity passes, or tends to pass, through it. Many powerful magnets are therefore arranged on a wheel, that they may be associated very near to another wheel, on which are fixed many helices with their cores like that described. Again, a third wheel consists of magnets arranged like the first; next to this is another wheel of the helices, and next to this again a fifth wheel, carrying magnets. All the magnet-wheels are fixed to one axle, and all the helix wheels are held immoveable in their place. The wires of the helices are conjoined and connected with a commutator, which, as the magnet-wheels are moved round, gathers the various electric currents produced in the helices, and sends them up through two insulated wires in one common stream of electricity into the lighthouse lantern. So it will be seen that nothing more is required to produce the electricity than to revolve the magnet-wheels. There are two magneto-electric machines at the South Foreland, each being put in motion by a two-horse power steam-engine; and, excepting wear and tear, the whole consumption of material to produce the light is

the coke and water required to raise steam for the engines, and carbon points for the lamp in the lantern.

The lamp is a delicate arrangement of machinery, holding the two carbons between which the electric light exists, and regulating their adjustment; so that whilst they gradually consume away, the place of the light shall not be altered. The electric wires end in the two bars of a small railway; and upon these the lamp stands. When the carbons of a lamp are nearly gone, that lamp is lifted off and another instantly pushed into its place. The machines and lamp have done their duty during the past six months in a real and practical manner. The light has never gone out through any deficiency or cause in the engine and machine house; and when it has become extinguished in the lantern, a single touch of the keeper's hand has set it shining as bright as ever. The light shone up and down the Channel, and across into France, with a power far surpassing that of any other fixed light within sight, or anywhere existent. The experiment has been a good one. There is still the matter of expense and some other circumstances to be considered; but it is the hope and desire of the Trinity House, and all interested in the subject, that it should ultimately justify its full adoption.

XLII. *Intelligence and Miscellaneous Articles.*

BORACIC ACID IN THE SEA-WATER ON THE COAST OF CALIFORNIA.

THE following interesting paper on boracic acid in the sea-water of the Pacific, on the coast of California, was read by Dr. John A. Veatch before the California Academy of Natural Sciences. The facts presented may lead to important results in various ways, and deserve attention from scientific men. The Doctor said,—

The existence of boracic acid in the sea-water of our coast was brought to my notice in July 1857. I had, in the month of January of the previous year, discovered borate of soda and other borates in solution in the water of a mineral spring in Tehama county, near the upper end of the Sacramento valley. Prosecuting the research, I found traces of boracic acid—in the form of borates—in nearly all the mineral springs with which the State of California abounds. This was especially the case in the Coast mountains. Borate of soda was so abundant in one particular locality, that enormous crystals of that salt were formed at the bottom of a shallow lake, or rather marsh, one or two hundred acres in extent. The crystals were hexahedral with beveled or replaced edges, and truncated angles; attaining the size, in some cases, of 4 inches in length by 2 in diameter, forming splendid and attractive specimens. In the same neighbourhood, a cluster of small thermal springs were observed holding free boracic acid in solution. A few hundred yards from these a great number of hot springs, of a temperature of 212° Fahr., rose up through the fissures of a siliceous rock. These springs held a considerable quantity of borax, as well as free boracic acid. Many other localities furnished similar indications, but in a less extensive form.

In progress of the examination I found that the common salt (chloride of sodium) exposed for sale in the San Francisco market, and which, it was understood, came from certain deposits of that

article on the sea-margin in the southern part of the State, also furnished boracic acid. I was led to attribute it to the fact of mineral springs emptying into the lagoons furnishing the salt. It was therefore a matter of no small surprise when, on a visit to the localities, I found no trace of acid in any of the springs in the adjacent district. This led to an examination of the sea-water, and a detection of an appreciable quantity of boracic acid therein. It was at Santa Barbara where I first detected it, and subsequently at various points, from San Diego to the Straits of Fuca. It seems to be in the form of borate of soda, and perhaps of lime. The quantity diminishes towards the North. It is barely perceptible in specimens of water brought from beyond Oregon, and seems to reach its maximum near San Diego.

This peculiarity seems to extend no great distance seaward. Water taken thirty or forty miles west from San Francisco gave no trace of acid. In twelve specimens, taken at various points betwixt this port and the Sandwich Islands, furnished me by Mr. Gulich, of Honolulu, only that nearest our coast gave boracic acid. In ten specimens kindly furnished me by Dr. W. O. Ayres, taken up by Dr. J. D. B. Stillman, in a trip of one of the Pacific mail steamers from Panama to this place, no acid was observed south of the Cortez Shoals.

I have not as yet been able to obtain specimens of water south of San Diego, nearer the shore than the usual route of the mail steamers. Neither have I been able to test the breadth of this boracic acid belt any further than the fact above stated, of no acid being found at the distance of thirty or forty miles west from the Golden Gate. I think it probable that it is confined within the submarine ridge running parallel with the coast, the southern portion of which is indicated by certain shoals and island groups. The source of the acid is undoubtedly volcanic, and the seat of the volcanic action is most likely to exist in this submerged mountain range. It strengthens the probability of the eruptive character of the Cortez Shoals.

I hope hereafter to be able to make more accurate and extended examinations, unless some one more capable of doing justice to the subject should take it in hand. With this view, I solicited the attention of Dr. J. S. Newberry to these facts, while he was in this city, on his way to join Lieut. Ivés's Colorado Exploring Expedition, hoping he might think it worthy of investigation during his stay on this coast. With the same view, I now submit them to the Academy. —*Journal of the Franklin Institute* for February 1860.

A NEW KIND OF SOUND-FIGURES FORMED BY DROPS OF A LIQUID.
BY F. MELDE.

If a drinking-glass, or a funnel of about 3 inches diameter at the edge, be filled with water, or alcohol, or ether, and a strong note be made by drawing a violin-bow on the edge, a sound figure will be formed on the surface of the liquid, consisting of nothing but drops of liquid. If the vessel gives the fundamental note, the figure forms a four-rayed star, the ends of which extend to the four nodal points; but if the note which the vessel gives be the second higher, the star will be six-rayed; and if the vessel gives still higher tones, other more numerously rayed stars are produced.—Poggendorff's *Annalen*, January 1860.

XLIII. *Crystallographic Notices.* By W. H. MILLER, *M.A.,*
F.R.S., Professor of Mineralogy in the University of Cambridge*.

On the Employment of the Stereographic Projection of the
Sphere in Crystallography.

IN the Philosophical Magazine for July 1859, it is shown that
the properties of anharmonic ratios may be used with
advantage in constructing the gnomonic projection of the sphere
by which Neumann represents crystalline forms, and also in
constructing the projection applied to the same purpose by
Quenstedt. I was unable, at that time, to extend the method
to the representation of crystalline forms, according to Neumann's
method, by the stereographic projection. Subsequently, however,
I have ascertained that it is equally applicable to the stereographic
projection, leading to a construction by which the centre of the
projection of any zone-circle may be readily determined. Hence,
having given the centre and radius of the primitive, the radius
of the projection of a zone-circle may be found, being the hypo-
thenuse of a right-angled triangle one side of which is the radius
of the primitive, and the other side the distance of the centre of
the projection of the zone-circle from that of the primitive. The
construction for finding the magnitude of a line or angle from
that of the anharmonic ratio into which it enters, will be found
in art. 13 of the paper in the Philosophical Magazine referred
to above.

Let P, Q, R, S be the centres of the projections of four zone-
circles KP, KQ, KR, KS passing through the point K; K the
projection of K; efg, pqr the symbols of KP, KR; hkl,
uvw the symbols of the poles Q, S in the zone-circles KQ, KS.

The angle which the distance between the centres of any two of the circles subtends at K, is equal to the angle between the corresponding originals. Therefore, since the anharmonic ratio of P, Q, R, S is the same as that of K P, K Q, K R, K S,

$$\frac{PQ}{RQ}\frac{RS}{PS} = \frac{eh+fk+gl}{ph+qk+rl}\frac{pu+qv+rw}{eu+fv+gw}.$$

Let the zone-circle $Q\,S$ meet the zone-circles $K\,P$, $K\,R$ in the poles P, R. Let T be the centre of the projection of the zone-circle $Q\,S$;

$T\,P$, $T\,Q$, $T\,R$, $T\,S$ the loci of the centres of projections of great circles the originals of which pass through P, Q, R, S respectively. Therefore, since the anharmonic ratio of the lines $T\,P$, $T\,Q$, $T\,R$, $T\,S$ is the same as that of the points P, Q, R, S,

$$\frac{\sin PTQ}{\sin RTQ}\frac{\sin RTS}{\sin PTS} = \frac{eh+fk+gl}{ph+qk+rl}\frac{pu+qv+rw}{eu+fv+gw}.$$

The symbol of any zone-circle may be used to denote the centre of its projection, and the symbol of any pole may be used to denote the straight line which is the locus of the centre of the projection of a great circle passing through it.

Let D, E, F, G be the centres of the projections of four zone-circles, no three of which are in one straight line; H the intersection of D E, F G; M the intersection of the circles having their centres in D, E; N the intersection of the circles having their centres in F, G. The straight line D E is the locus of the centres of the projections of great circles passing through the original of M; F G is the locus of the centres of the projections of great circles passing through the original of N. Therefore H is the centre of the projection of the great circle which is the original of M N. Hence, if the centre of the projection of a zone-circle be denoted by the symbol of the original, and the line joining any two centres be denoted by the symbol of the pole in which the originals intersect, the rule for finding the symbol of a zone-circle from the symbols of two poles in it, or for finding the symbol of the pole in which two zone-circles intersect, from the symbols of the zone-circles, may be applied to find the symbol of a line from the symbols of two centres through which it passes, or to find the symbol of the intersection of two lines each of which joins two centres, from the symbols of the lines.

The expression

$$\frac{eh+fk+gl}{ph+qk+rl}\frac{pu+qv+rw}{eu+fv+gw},$$

where $e\,f\,g$, $p\,q\,r$ are the symbols of two zone-circles $K\,P$, $K\,R$,
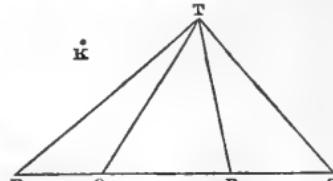
or two straight lines K P, K R, and *h k l*, *u v w* are the symbols of two poles Q, S or of two points Q, S, may be conveniently denoted by K P, Q . K R, S (or Q, K P . S, K R), which shows how the indices of K P, K R, Q, S are combined in the numerator. This notation is especially useful when the indices of K P, Q, K R, S are not denoted by letters. When they are denoted by letters, it suggests *e f g*, *h k l . p q r*, *u v w* as a convenient abbreviation of the preceding expression.

Let D, E, F, G be four centres of projections of zone-circles, no three of which are in one straight line, and of which the symbols are known; T the centre of the projection of any other zone-circle. Let H be the intersection of D E, F G. The symbols of D, E being given, that of D H is known. When the symbol of T is given, that of D T may be found. The angle G D T is then given by the equation

$$\frac{\sin \text{GDH}}{\sin \text{FDH}} \frac{\sin \text{FDT}}{\sin \text{GDT}} = \text{G, DH . F, DT.}$$

In like manner the angle G E T is given by the equation

$$\frac{\sin \text{GEH}}{\sin \text{FEH}} \frac{\sin \text{FET}}{\sin \text{GET}} = \text{G, EH . F, ET}$$

Hence the position of T is determined.

When T is given, the ratios of the indices of the zone-circle the projection of which has T for its centre may be found from the preceding equations.

Having given the symbol of a pole, to find the centres of the stereographic projections of any two great circles which intersect in its projection Q.

Let the locus of the centres of the projections of great circles passing through Q, meet D E in V, F G in W, and G D in U. The symbol of U W is the same as that of the original of Q. From this, and the symbols of D, E, F, G, those of H, U G, U W, can be obtained. The points V, W are then given by the equations

$$\frac{\text{HD}}{\text{ED}} \frac{\text{EV}}{\text{HV}} = \text{H, UG . E, UW,}$$

$$\frac{\text{HG}}{\text{FG}} \frac{\text{FW}}{\text{HW}} = \text{H, UG . F, UW.}$$

When Q, the projection of any pole, is given, let the straight line U W passing through the centres of the projections of any two great circles intersecting in Q, meet D E, F G in V, W. Then the preceding equations give the ratios of the indices of U W, or of the original of Q.

It is easily seen that the centre of the stereographic projection of a zone-circle is Quenstedt's projection of the corresponding zone-axis, the nearer pole of the primitive being the fixed point of Quenstedt's projection; and that the straight line through the centres of the stereographic projections of two zone-circles, is Quenstedt's projection of the face having its pole in the intersection of the two zone-circles.

On the Measure of the Dihedral Angles of Crystals.

Euclid's definition of a dihedral angle takes no account of the difference in the nature of the matter on opposite sides of the planes forming the dihedral angle; therefore, though sufficient for the purposes of geometry, it must be modified to suit the requirements of crystallography. The dihedral angle made by two faces of a crystal, considered as planes separating matter of one kind from matter of another kind, may be measured in two different ways; either by the angle between normals to the faces, drawn from any point within the crystal towards the faces; or, by the supplement of this angle. The latter measure, which was unfortunately adopted by the earlier crystallographers, leads to the preposterous conclusion, that if two plane mirrors be placed back to back, with their faces perpendicular to a given straight line, the angle which the face of one mirror makes with itself is 180°, and the angle which the face of one mirror makes with that of the other, is 0°, though the mirrors are in the most dissimilar positions, having their faces directed to points diametrically opposite. It is scarcely possible that this measure would have been adopted if the invention of the Reflective Goniometer had preceded the crystallographic researches of Romé de l'Isle. In order to give an angle by a single reading, in accordance with Carangeot's goniometer, Wollaston repeated the numbering of the graduation in each semicircle (a source of ambiguity in the recorded observations), instead of numbering up to 360°, as is usual in circular instruments, and introduced two stops and a spring which permitted the circle to turn only in a direction contrary to that of the numbering, and enabled the circle to be fixed nearly at 0° and 180°. This contrivance was but partially successful; for it only gave the angle between two faces one of which was observed at 0° or 180°, leaving the other dihedral angles to be obtained by subtracting the difference between the corresponding readings from 180°; and in the most carefully constructed instruments the adjustment of the stops was too uncertain to fix the zero of the vernier at 0° or 180°, without leaving an error too large to be neglected. In the best goniometers now constructed the stops are omitted, and the graduation is numbered up to 360°. The difference of the readings

of the circle, corresponding to the observations by reflexion from any two faces of a crystal, gives the angle between normals to the faces, from a point within the crystal. The data employed in calculating dihedral angles, and the results of the calculations, are expressed in the same measure. The supplement of the angle between normals to two faces is nowhere used as the measure of a dihedral angle, except in the lists of angles which accompany the descriptions of mineral species. An angle taken from one of these lists cannot be compared with the direct result of observation or of calculation, without first subtracting one or the other of them from 180°. In order to avoid the needless trouble of subtracting angles from 180°, which from its frequent occurrence becomes extremely irksome, the editors of the last edition of Phillips's 'Mineralogy' ventured to measure a dihedral angle by the angle between normals to the faces containing it, from a point within the crystal. That they have not been over hasty in breaking through an inconvenient and unphilosophical convention, may be gathered from the fact that the same definition has since been adopted by Beer, Dauber, Grailich, Guiscardi, Handl, Hess, v. Lang, Murmann, Rotter, Schröder, Sella, de Sénarmont, and v. Waltershausen.

The use of the angle between normals to the faces as the measure of the dihedral angle they make with each other, is attended by some incidental advantages. It enables the reader to apprehend more clearly the relative positions of the faces by inspection of the recorded angles; employs fewer figures; and in the descriptions of twin crystals, marks re-entering angles by giving them negative values.

On the Cleavages of Rutile.

In Breithaupt's 'Mineralogy' rutile is described as having cleavages parallel to the faces of the forms 1 0 0 and 1 1 0, with traces of cleavage parallel to the faces of the form 1 1 1. The last of these has been overlooked in the mineralogical treatises which have appeared since 1847, the date of the last volume published of Breithaupt's work. Three crystals of rutile forming part of the Brooke Collection, now in the Mineralogical Museum of Cambridge, exhibit the cleavage 1 1 1 very distinctly. In two of them it is interrupted by traces of cleavage parallel to the faces of the form 3 2 1. I have also observed the cleavage 1 1 1 in two crystals of rutile in my own possession, and in one of them, rather obscure traces of the cleavage 3 2 1. The symbol 1 1 1 is used to denote the simple form in which 1 1 1, 1 1 $\bar{1}$ = 95° 20′; 1 1 1, $\bar{1}$ 1 1 = 56° 52′. 3 2 1, 0 0 1 = 66° 42′; 3 2 1, 2 3 1 = 20° 46′; 3 2 1, 1 1 1 = 26° 0′.

On the doubly-refractive character of Thermophyllite.

Some crystals of thermophyllite, a mineral of which an analysis by Mr. Northcote was published in the Philosophical Magazine for October 1858, were too imperfect to be measured, or to exhibit coloured rings when examined with a polarizing instrument having three lenses of equal focal length, resembling the instrument contrived by the Astronomer Royal (Cambridge Philosophical Transactions, vol. iv. p. 199). Nörrenberg's newest polarizing instrument, in which the focal length of the eyepiece is many times that of the other two lenses, or combinations of lenses (Grailich, *Krystallographisch-optische Untersuchungen,* p. 43), though it does not allow the positions of the optic axes to be determined with much accuracy, permits the use of a proportionably smaller slice of crystal. With an instrument constructed on this principle it was not difficult to make out the existence of two optic axes in thermophyllite, making with each other an angle of about 22° 30′ in air. The position of the bisectrix with respect to the cleavage could not be measured on account of the curvature of the latter. It appeared to be perpendicular to the cleavage. It is therefore probable that the crystals belong to the prismatic system.

XLIV. *On the Composition of Water obtained from the Coal-strata, Bradford Moor, Yorkshire.* By F. A. Abel, *Esq.*[*]

THE analysis of a sample of water from the above source was undertaken a short time since with the view to ascertain whether it was adapted to general domestic purposes. The results furnished by the examination appeared of sufficient interest to warrant their publication.

Two samples of the water collected at the mouth of a coal-pit, at an interval of about one month (the separate analyses of which furnished thoroughly concordant results), were submitted to me officially for examination by Lieutenant Colonel Hamley, Commanding Royal Engineer at York, who informed me that the water, which is highly esteemed in the neighbourhood for drinking and culinary purposes, is raised from coal-pits, at a depth of about 200 feet beneath Bradford Moor,—an abundant and regular supply being obtained.

The specific gravity of the water was 1000·78 at 60° F. Its reaction was powerfully alkaline, and its flavour was brisk and agreeable.

The proportion of solid matter obtained on evaporation

amounted to 44·1 grains in an imperial gallon, of which by far the largest proportion consisted of carbonate of soda.

The alkalinity of the boiled water was determined by means of standard sulphuric acid, and found to be equivalent to a proportion of 30·76 grains of carbonate of soda in an imperial gallon.

The result obtained by the direct determination of the carbonic acid, corresponded accurately to the proportion required by theory to hold in solution the whole of the lime and magnesia in the water, and to form bicarbonate with the amount of soda represented by the number above quoted.

The following statement represents the proportions of the various constituents existing in solution in an imperial gallon of the water:—

Bicarbonate of soda	43·53
Sulphate of soda	7·50
Chloride of sodium	1·34
Sulphate of potassa	0·31
Phosphate of lime	trace
Carbonate of lime	1·90
Carbonate of magnesia	0·80
Organic matter	1·20

Carbonic acid, holding the carbonates of lime and magnesia in solution, 1·25 grain = 2·642 cubic inches at 60° F.

The absence of nitric acid, ammonia, silicic acid, alkaline sulphides, and oxide of iron was established by special examinations.

XLV. *Note on the Remarks of* Mr. Jerrard.
By James Cockle, *Esq.*[*]

THE inverses of the rational functions, say R, by which one of two similar functions is expressed in terms of the other are themselves rational, and the inverses of those by which one root of an irreducible equation is (if so expressible) expressed rationally in terms of another are also themselves rational. And if Ξ, θ be similar functions of which Ξ_1, θ_1 and Ξ_2, θ_2 are corresponding values, and if moreover Ξ be the root of an irreducible equation one root of which, Ξ_2, is a rational function, say r, of another, Ξ_1, we find

$$\theta_2 = R\Xi_2 = Rr\Xi_1 = RrR^{-1}\theta_1,$$

in other words, that θ_2 is a rational function of θ_1. Consequently if the equation in θ is not an Abelian, neither is the equation in Ξ an Abelian.

Again: if a rational equation be reducible, any rational transformation involving only one root gives rise to a reducible trans-

formed equation.   And, since

$$W_{f} + W_{f\,(\beta,\epsilon)}^{\bullet}, \text{ and } \theta_1 + \theta_4, \text{ and } \theta_1\theta_4$$

are similar functions, if the 15-ic in the V of Mr. Jerrard be reducible to cubic factors, the 15-ics in $\theta_1 + \theta_4$ and $\theta_1\theta_4$, that is to say in ξ and δ, are so reducible.   But this is not the case. Under the most favourable circumstances in which we can form the cubic in ξ, the coefficients are unsymmetric.   And the structure of the 15-ic in γ, which is reducible to a quintic and a 10-ic equation, discloses no means of attaining a cubic with known coefficients.   The most favourable combinations, those of the forms $\gamma_5\beta_5\alpha_5$, $\gamma_5\beta_4\alpha_1$, or $\gamma_5\beta_2\beta_3$*, are unsymmetric.

Further : the coefficients of the cubics of Mr. Jerrard are (see arts. 69, 94, 109, and 110 of his 'Essay†') expressible rationally in terms of x_1, x_2, .. x_5, and the doctrine of similar functions shows that they are either symmetric or incapable of evaluation save by a quintic.   In the former case the five cubics are identical ; in both cases the results are illusory.   It is a significant fact that the soluble form of art. 96 of my 'Observations,' for which the sextic in t degenerates into a cubic, is not irreducible‡.

The 'Essay' of Mr. Jerrard is of surpassing interest, but these objections to the particular portion of it which relates to the finite solution of quintics seem to me to be fatal.   A deep admirer of his researches, and indisposed to regard as established conclusions in which Mr. Jerrard does not concur, I may be permitted to express a hope that the promised sequel to the 'Essay' will not be long delayed.

Lastly : how can each one of the system of five cubics mentioned in art. 110 (p. 84) of Mr. Jerrard's 'Essay' be separated from the rest save by a quintic ?   How can this quintic be solved unless it be an Abelian ?   And how can it be an Abelian if the given quintic be not an Abelian ?   What evidence is there that the four N's vanish as alleged by Mr. Jerrard ?

4 Pump Court, Temple, London, E.C.,
    April 7, 1860.

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* Mr. Harley has completely determined all the β and α functions.   I have selected these combinations from his values, kindly communicated to me.

† Part I. 1858; Part II. 1859.   Taylor and Francis.

‡ See a solution, for the case Q=1, by Mr. Stephen Watson in the 'Educational Times,' April 1860.   Neither of the standard particular solvable forms

$$x^5 - \frac{5}{2}x^2 + \frac{3}{2} = 0, \quad x^5 - 5x^2 + 2 = 0$$

to which I have been conducted is irreducible.

XLVI. *Note on some Prismatic Forms of Calcite from Luganure, County of Wicklow.* By WILLIAM K. SULLIVAN[*].

IN the first edition of his *Traité de Minéralogie* (Paris, 1801), Haüy distinguished three kinds of prismatic carbonate of lime:—1. *Chaux carbonatée prismée,* already described by Romé de Lisle, and which Haüy supposes to be derived, in his molecular theory of decrements by the law d^1. According to this, it would be the prism produced by modifying planes placed upon the lateral edges of the primitive rhombohedron. The second he calls *chaux carbonatée imitative,* and considers to be the prism obtained according to the law e^2 by planes on the lateral angles of the primitive. The third, which had also been before described by De Lisle, he named *chaux carbonatée prismatique,* and considered to be also derived according to the law e^2. He mentions four varieties of this form: *a,* alternating—having three alternate wide faces and three intermediate narrow ones; *b,* compressed—with two opposite faces larger than the other four; *c,* widened—with four faces wider than the remaining two; and *d,* lamelliform—in very short (*i. e.* in tabular) prisms. Of the crystals of this form he says, " In certain crystals the extremities are of a dull white, while the intermediate part is transparent. In others the opake part is situated towards the axis and surrounded by a transparent envelope. The bases of a few exhibited concentric hexagons, and one could even observe the extremity of a small internal prism, rising above the whole prism."

The forms he calls imitative and prismatic being obtained by the law e^2, contain the same prism; the prismatic faces which have been observed among the varieties of calcite belong, therefore, to one or other of those prisms. Dufrénoy, who uses the nomenclature of Haüy, as modified by Levy and himself, represents the faces of the first prism, or that on the edges of the rhombohedron, by the symbol d^1 (*u* of Haüy), and the prismatic, or that on the angles, by e^2 (*c* of Haüy). Of course each of these prisms is completed by the modification a^1 on the summit angle, which produces the horizontal plane forming the base.

According to the German crystallographic methods, prisms are looked upon as mere limiting forms. Mohs and Haidinger consider d' to be the limiting form of the pyramids, the former expressing it by the symbol $P + \infty$ and the latter by ∞P, which is the one adopted by Zippe in his summary of all the observed forms of carbonate of lime [†]. The second prism, e^2, is considered

to be the limiting form of the rhombohedron, and is represented by Mohs by the symbol $R + \infty$, and by Haidinger by ∞R. Zippe also adopts the latter.

According to Haüy d' or ∞P is rare, and Dufrénoy states that only some examples are known. According to Zippe, it is frequent enough in combination as a secondary form, but seldomer as the dominant form. Surmounted by the primitive rhombohedron (R or P), it is noticed by Dufrénoy as "a very rare example of the prism on the edges, associated with the primitive rhombohedron*" from Cumberland. He also mentions another in which b' or $\frac{1}{2}R'$ (the *équiaxe* of Haüy) replaces P or R, but does not give the locality. Further on he notices a third example from the Samson mine in the Hartz, in which the horizontal edges of the prism are truncated by rudimentary planes of the pyramid.

The prism ∞R or e^2, although comparatively rare as a simple form, is very frequent in combination; according to Dufrénoy, indeed, it is the only one found complete. A little before, he says that it is of a milky whiteness, and almost always opake. The base sometimes bears striæ parallel to the edges, which are indications of cleavage. Examples of ∞R surmounted by $\frac{1}{2}R'$ or b' from the Hartz, Cumberland, and the department of l'Isère, have been described.

The position of the rhombohedrons surmounting the prisms is different in each kind. In ∞P the surmounting rhombohedral faces lie so that the edges of combination with the prismatic faces coincide with the lateral edges of the rhombohedron. In ∞R the edges of combination in three alternate faces are horizontal; the truncatures at either end of the prism alternating, so that each face of truncature is parallel to one at the opposite end. The directions of the cleavages correspond perfectly with the dispositions of the modifying planes, so that every alternate basal edge of the prism ∞R or e^2 may be removed by cleavage with the greatest facility, by which a prism surmounted by the faces of the rhombohedron may be obtained.

Although the prismatic faces ∞R are sometimes dull, they always, at least in all the crystals which I recollect to have seen, possess more lustre than the faces ∞P associated with them. The former are, indeed, usually very bright in transparent crystals. This circumstance is noticed by Dufrénoy, who, in speaking of the example of ∞P or (d^1) with pyramidal truncatures of the lateral edges, from Samson mine in the Hartz,

* *Traité de Minéralogie*, par A. Dufrénoy. 2 ed. tome ii. p. 297.

says that the faces are dull and somewhat rough, as is frequently the case with those prisms ["les faces en sont mates et un peu raboteuses, circonstance fréquente pour le second prisme (*i. e. d¹*) à six faces"]. The difference in lustre between the faces of the two kinds of prisms is characteristically seen in the dodecagonal prisms (chaux carbonatée péridodécaëdre of Haüy), which is the combination $\infty R, \infty R,$ oP ($d^1\ e^2\ a^1$); the faces ∞R (e^2) are always very much more brilliant than ∞P (d^1). This difference of lustre is one of the distinctions relied upon to distinguish the faces of the two kinds of hexagonal prisms from one another. Dufrénoy also notices this difference between the two kinds of prismatic faces in the twelve-sided prisms.

Several forms of the rhombohedral prism occur at the Luganure mines, County of Wicklow, which are worked for galena in a veinstone consisting chiefly of quartz, in a granite country. Among these may be mentioned ∞P, oR (d^1, a^1), consisting of small hexagonal prisms, with very bright prismatic faces. One half of the prism is hyaline, and the other opalescent; the base oR is dull. Another variety of the same form also occurs, consisting of crystals one centimetre high, and with basal edges one centimetre long. Each crystal has a sort of rude triangular prismatic milky nucleus, surrounded by a perfectly hyaline envelope, reminding one of the description of Haüy given above. Owing to the number of cleavage planes, some crystals are not transparent. The face oR is in most instances peculiarly striated, in others it is, as it were, coated with a thin porcelaneous layer. These crystals may be easily cleaved parallel to the alternate basal edges, which are sharp, and without any trace of modifying planes. The form oR, ∞R (a^1, e^2) also occurs in beautiful hexagonal plates, with very bright prismatic faces, and composed of exceedingly thin alternating layers of white opake, and hyaline matter, the base oR being always opake, dull, but beautifully white. Haüy's description of the prismatic kind embraces this variety likewise; in fact, the specimens from Luganure here described illustrate perfectly Haüy's description.

I have lately, however, met with another form, consisting of hexagonal plates, of from one millimetre to one and a half thick, with basal edges of from five to twenty millimetres. The base has a bright nacreous lustre, much brighter than what I have ever seen in any other specimen; striated and uneven, in consequence of the lapping of smaller plates. The most of the tabular prisms are, in fact, compound twins to the base oR (a^1). Some twins also occur to the faces of the prism, and finally, to a rhombohedron. It is owing to this twin structure that the crystals are not generally transparent; for in thin plates they are

perfectly hyaline. Except for the difference of form, a mass of these crystals, resting on crystalline quartz, resembles, in a striking manner, a mass of large crystals of chlorate of potash. Layers of growth in the direction of the secondary axes can be observed in some of the prisms ; in many of these the outer shell, about one millimetre thick, is frequently free from indications of cleavage, and perfectly transparent. The prismatic faces are dull, exactly like the appearance of white wax when sufficiently thin to be translucent; they are also uneven. These faces exactly resemble those of the prism ∞P (d^1), in specimens which I have seen from Andreasberg. On this account, I concluded, at first sight, that I had the combination oP, ∞P, which would be not merely rare as an example of the pyramidal prism, but still more so as a tabular form of it, in which the base would impress its character upon the crystal, and of which I have not seen any example recorded. I found, however, that the alternate edges were modified by rudimentary facets of a rhombohedron, which was placed in the same position, as regards the faces of the prism, that I have before mentioned as characteristic of ∞R. The basal edges not modified were easily removed by cleavage. I found the modifying facets to be those of the rhombohedron $\frac{1}{2}$ R$^{\prime}$ or b^1.

Associated with the crystals just described, were sometimes found white opake crystals, like those from Andreasberg, and others three or four millimetres thick, upon which were rudimentary facets of a scalenohedron. I have not been able to get any good specimens of these varieties.

It may be worth while to enumerate, from Zippe's excellent memoir, the tabular prismatic forms which have been hitherto observed, with a view of determining the exact position of the example just described in the series. They are as follow :—

1. oR, $2P$, ∞R, ∞P (a^1, e^3, e^2, d^1) figured by Levy[*].
2. oR, $\frac{1}{2}R^{\prime}$, ∞R (a^1, b^1, e^2) white tabular crystals from Weardale in Durham.
3. oR, ∞R, ∞P (a^1, e^2, d^1) from Andreasberg.
4. oR, $2R^{\prime}$, ∞R, ∞P (a^1, e^1, e^2, d^1) from Andreasberg.
5. oR, $\frac{1}{4}R^{\prime}$, ^{3} R, ∞P from Andreasberg.
6. oR, ∞R from Andreasberg, Marienberg, Schneeberg, Joachimsthal, and Schemnitz.

The last-mentioned form from Luganure, which is oR, ∞R, $\frac{1}{2}R^{\prime}$ (a^1, e^2, b^1), approaches nearest to No. 6, from which it differs, so far as can be expressed by a formula, only by the rudimentary rhombohedral facets. If the faces $\frac{1}{2}R^{\prime}$ became so developed as to

* *Description d'une collection de minéraux formée par* H. Heuland, &c. Londres, 1837, fig. 87.

render the faces ∞R subordinate to them, it would pass into the form No. 2 from Weardale. I have, indeed, found a few imperfect crystals from Luganure, in which the prismatic faces are only rudimentary, the outline of the tabular crystal being rhombohedral.

Although, as I have above observed, the prismatic faces ∞R are sometimes dull, the combination of brilliant nacreous oR faces with wax-like prismatic faces exactly like those characteristic of the faces ∞P is, so far as I am aware, extremely rare. In the mineralogical collection of the Museum of Irish Industry there is a specimen from Andreasberg, in tabular crystals somewhat thicker than those from Luganure, which I have described. The same kind of rudimentary facets occur in the alternate basal edges. I have not had an opportunity of determining whether they belong to $\frac{1}{2}R'$ (b^1). The prismatic faces have the wax-like dullness of the Luganure specimens; but the crystals are opake, and the faces oR are dull, and in other respects very different in appearance from those just mentioned. In the same collection, characteristic specimens of the other forms from Luganure which I have mentioned are to be found, as well as of several others, of which I have not yet been able to procure specimens*.

* It is to be regretted that the descriptions, both crystallographic and mineralogical, of the minerals from Irish localities, which are to be found in Irish collections, have not been more generally published. It is only by the careful study of the conditions under which certain forms of minerals are found, the first element of which is a faithful record of the circumscribed localities in which they occur, that we can hope to arrive at a solution of the important problem in molecular physics—the causes which produce modifications of form in bodies. The ' Manual of the Mineralogy of Great Britain and Ireland,' by Robert Philips Greg, F.G.S., and William G. Lettsom, forming one of the admirable series of Manuals published by Van Voorst, is a most praiseworthy step in this direction. It is with regret, however, that I have to state that this otherwise excellent and useful work is full of errors regarding Irish localities,—errors, too, of the strangest kind, not mineralogical, but geographical, and which one would scarcely expect to find made respecting the divisions of an Asiatic country. I do not speak of such errors as *Rovenagh* and *Borenagh* for Bovevagh (pp. 54 and 88), *Bum Beg* for Bun Beg (p. 101), or *Glen Maceness* for *Glenmacnass*, which are, however, too numerous to be pardonable, but of such errors as County of *Cavenagh* for County of *Cavan* (p. 20); " Ballygahan mine, at Glandore, County of Wicklow " (p. 279), Glandore being in the County of Cork; " Knockmahon and Tigroney in Waterford " (p. 305), Tigroney being in Wicklow; " In Wicklow, at Audley mine " (p. 311), Audley mine being in the County of Cork. I hope a second edition will enable the authors, not only to correct these errors, but to greatly extend the list of localities.

XLVII. *On certain Inductions with respect to the Heat engen-
dered by the possible Fall of a Meteor into the Sun; and on a
mode of deducing the absolute Temperature of the Solar Surface
from Thermometric Observation. By* J. J. WATERSTON, *Esq.*[*]

MR. CARRINGTON'S observation of the sun on the 1st of
September last having fortunately established the fact of
an outburst of light above the solar surface, and thus favouring
Newton's conjecture as to the sun receiving a supply of force
from bodies descending upon it, it may be worth while, and
perhaps assist in the formation of more exact ideas on the sub-
ject, if we compute, from Mr. Joule's value of the thermal unit,
the quantity of heat and intensity of temperature that would
accrue by a body falling into the sun or sun's atmosphere with
the velocity due to a parabolic trajectory. The modern inter-
pretation of Newton's conjecture is, that the comet, or by what-
ever name we call the body, does not supply fuel to a fire as oil
to a flame, but that it supplies force to the central radiating
energy—force to be converted into heat and light.

It is true that no body belonging to the solar system can fall
into the sun so long as the laws that keep them in their orbits
are maintained. A body that has once rounded the sun in a
parabola or elongated ellipse, will probably continue to do so,
unless in the rare case of the perihelion distance being so close
to the sun's surface that a slight perturbation from the other
bodies of the system may bring it within the limits of the sun's
radius. But when the orbit is a hyperbola, it is the first and
last appearance of the body; and if, as may chance to happen,
the perihelion distance is less than the sun's radius, the sun must
inevitably absorb it. We are thus certain that the velocity of a
body that impinges on the sun must exceed 419 miles per second;
for such is the orbital velocity in the parabola that separates the
lesser velocity in the ellipse from the greater velocity in the hy-
perbola.

With this velocity and Mr. Joule's unit it is easy to compute
the quantity of heat due to the conversion of the force; and if
the absolute zero of temperature coincide with −461° F., the
zero of gaseous tension, the temperature attainable by a known
substance under such conditions may also be exactly computed,
independent of any theory or hypothesis whatever.

A pound of water falling through 772 feet acquires a velocity
of 222 feet per second, and a force that, if converted, raises the
temperature of the pound of water 1° F. If it falls through four
times 772, the velocity acquired is twice 222 feet, and the rise
of temperature 4°. Suppose the acquired velocity to be 1 mile

[*] From the Proceedings of the Astronomical Society for April 3, 1860.

per second, the corresponding rise of temperature is 564°, increasing as the square of the velocity; and 419 miles per second corresponds to $419 \times 419 \times 564 = 99,016,404°$.

A pound of iron under the same circumstances would acquire the same force; but that force converted would raise its temperature nine times the above amount, because the same quantity of heat has nine times greater effect in raising the temperature of iron than it has on the same weight of water.

A pound of mercury would have its temperature affected about thirty times the above amount, and so on according to the specific heat of the substance.

Assuming that the specific heat of the body that impinges on the sun is the same as glass, the rise of temperature corresponding to the velocity 419 miles per second is 565 million degrees Fahrenheit.

Thus the intensity of the temperature engendered depends on the molecular constitution of the body: the *quantity* of the heat, however, is independent of everything but the velocity and the mass; and each pound of any body whatever that strikes the sun with the velocity 419 miles per second is endowed with force sufficient to raise a pound of water 100,000,000 degrees of Fahrenheit's scale.

Has the mass of the sun been gradually collocated by matter thus descending? To estimate exactly the probability of this, two other data are required, viz. the temperature of the sun's surface, and the quantity of heat radiating from it in a given time. The first is unknown, but the second we know approximately from the observations of M. Pouillet, also from those of Herschel and Forbes. It is estimated that it suffices to melt a stratum of about 2 feet thickness of ice at the earth's mean distance in twenty-four hours. This is equivalent to 1·04 foot of ice per second at the sun's surface. Now it is known that ice requires 142° F. to melt it, and each of those degrees is equivalent to the work expended in raising the weight of the ice 772 feet against the force of gravity at the earth's surface. We thus deduce that a weight equal to that of the ice arriving at the sun with a velocity of 2649 feet, or half a mile per second (viz. the velocity acquired in falling through 142×772 feet), is equivalent to the force emanating from the sun in one second. If the same amount of force is brought to the sun by matter moving at the minimum velocity of 419 miles per second, the mass of this matter must be less than the mass of the ice, in the ratio of 1 to the square of twice 419. Such a mass is represented by a sphere of 11·068 miles in diameter, of the density of water.

Supposing the sun to have the same density and specific heat as water, and comparing its volume with that of the sphere of

11 miles diameter, and allowing for the time, we find the
sun's annual loss of temperature to be 6°·125. This is a simple
arithmetical deduction from the fact that the sun's heat can melt
about 2 feet of ice daily. The observations of Herschel and
Forbes fix it at 1·835 foot (Phil. Trans. 1842); those of M.
Pouillet at about one-third less (Taylor's Scientific Memoirs,
vol. iv.). If we assume 1·5 foot as the correct thickness, the
yearly reduction of temperature is 4°·59. If the specific heat of
the matter of the sun corresponded with iron, this decrement of
temperature would have to be multiplied by 9, and so on for
other assumed values. But it is convenient to found our ideas
of such quantitative relations on water as the standard most
familiar.

Assuming the earth to have six times the density of water, we
may extend these calculations; and comparing volumes and time,
we deduce 69 as the number of *years* that the sun takes to throw
out as much force as would accrue to it by the earth falling down
upon its surface. The mechanical force thus supplied would be
equivalent to the expenditure of heat-force for 69 years; and the
rise of temperature of the whole mass of the sun, supposing the
increment of heat uniformly diffused through its mass, would ap-
proach as much nearer to the maximum limit 317° ($=69 \times 4°·59$)
as the temperature of the planet after impact exceeded the tem-
perature of the sun.

Again, if we suppose the planet after it has struck the surface
of the sun to settle into a disc of 60,000 miles diameter, having
the temperature of 100,000,000 degrees, the temperature of the
sun being 12 millions, we should have $\frac{1}{200}$th of its disc shining
with eightfold lustre, giving out probably eight times as much
heat as an equal surface in the normal state. Thus $\frac{8-1}{220} = \frac{1}{31}$
represents the increment of solar heating power; and if we esti-
mate the normal amount as what is required to keep the earth's
surface at a mean temperature of 60° F. or 521° absolute, we
see that $\frac{521}{31} = 17°$ nearly, is the increment of mean temperature
over all the earth that would arise from such a planet-fall.

M. Pouillet infers from his observations on solar radiation,
that the temperature of the sun's surface is at least 2660° F.
If it do not exceed this amount, the rise of temperature in the
whole mass of the sun would be about 200°; but if, as before,
we assume the planet to settle into a disc 2′ in diameter, we
have $\frac{1}{220}$th of the sun's disc shining with 38,000 times the normal
force, so that a planet-fall of this magnitude would increase the
radiating power of the sun 171 times.

Such extravagant conclusions only show the insufficiency of our data, and demonstrate the uncertainty in which the subject is involved, so long as an approximation to the actual temperature of the solar surface is wanting.

Is it possible to ascertain the temperature of the radiating surface of the sun? Ordinary observations give us the temperature in the sun and in the shade. Suppose the temperature of the sun to be double its present amount, it is probable that the absolute temperature in the shade and in the sun would also be double the present amount; consequently, also, the difference between them. We might thus expect this difference, when due precautions are taken, to be a constant quantity, and to be a function of the sun's absolute temperature.

Suppose a thermometer to be enclosed in a vacuum and surrounded on all sides by matter having a uniform absolute temperature t: we may consider it to be the centre of a sphere, the interior surface of which radiates heat to it, and the balance of temperature to be thus maintained by reciprocal radiation,—as much power *issuing from* the thermometer on all sides towards the concave surface of the enclosing sphere as *enters* into it by radiation from the concave surface. There is a dynamic interchange of force in constant operation. If the temperature of the sphere is augmented one degree, the thermometer rises until its radiating power increases to the same amount. If half the concave surface remains at t, while the other half increases from t to $t+2°$, the rise in the temperature will be the same as before, viz. $1°$, because the supply to it is the same as if the whole surface were raised $1°$. If $\frac{1}{1000}$th of the surface had the temperature $t+1000°$, the other parts remaining at t, we have

$$\frac{1 \times (t+1000°) + 999 \times t}{1000} = t+1°,$$

the resulting temperature as before. If $\frac{1}{1000}$th of the surface had the temperature of $t+2000°$, then

$$\frac{1 \times (t+2000°) + 999 \times t}{1000} = t \times 2°$$

is the temperature of the thermometer; and generally, if $\frac{1}{n}$ of the surface of the sphere had the temperature $t+x°$, we have

$$\frac{(t+x°) + (n-1)t}{n} = t+\tau°,$$

the temperature of the thermometer. Hence $x° = n\tau°$, a simple relation, by which we can deduce the temperature of the sun's radiating surface, assuming for the present the non-absorption

of rays in passing through the atmosphere. It will be remarked that $\tau°$ is the difference of reading between thermometers in the sun and in the shade taken with due precautions.

Some years ago, when in India, I tried this by enclosing a thermometer within three concentric boxes well protected from external influences, and capable of being equally heated all round to any temperature below 400°, by means of flues ascending from an Argand lamp. The sun's rays, when near the meridian (having an altitude of about 70° and with the atmosphere perfectly clear and calm), were admitted to fall, when required, on the bulb of the thermometer through a narrow triplet glass partition. I found that 50° was the rise that took place in consequence of exposure. There being glass partitions on both sides, the reading of the thermometer was very distinct by transmitted light. Beginning at 80°, without applying the lamp, the admission of the sun's rays caused the mercury to rise to 130°, where it remained steady. The lamp was then applied at low power, so as to maintain the inner box at this temperature while the sun was excluded. When perfectly steady, the sun's rays were readmitted, and the mercury again mounted with the same alacrity as before, until it reached 180°. This continued step by step up to 250°. No difference, either in the magnitude of the step or the time taken to effect it, could be detected.

Thus, for $\tau°$ we have the constant 50°, and n we obtain by comparing the disc of the sun with the surface of the sphere. At the earth's mean distance the sun's diameter is $32'\ 3''\cdot6$, hence $n = 183960$ and $x = 918000°$.

If there is no fault in this mode of proceeding, we may with confidence estimate the solar temperature to be *above* 10,000,000 degrees, probably 12,000,000, allowing a reduction of one-third from absorption in passing through the atmosphere and the three plates of glass.

A notable fact, in making these observations, is that the step $\tau°$ seems wholly independent of the temperature of the medium in which the thermometer lies. Why this should be, is apparent from the equation. Substituting $2t$ for t, and consequently $x - t$ in place of x, is tantamount to heating the box from 80° up to 620°. Let τ' represent (in degrees) the step at this higher temperature $2t$, we have

$$\frac{2t + x - t + (n-1)2t}{n} = 2t + \tau' \text{ and } x - t = n\tau' \text{ and } \frac{\tau'}{\tau} = \frac{x-t}{x}.$$

Thus the step diminishes only about the $\frac{1}{18000}$th part, or $\frac{1}{360}$th of a degree during a change of upwards of 500° in the box.

From Mr. Carrington's observations it appears that the burst of light was of *much* greater intensity than the sun's normal

surface. This is quite consistent with its assumed temperature, which is much less than any probable estimate we can make of the temperature of the conversion of the force of a body striking the sun's atmosphere with a velocity of from 400 to 500 miles per second. The existence of a transparent atmosphere seems also to be positively demonstrated by the blaze occurring above the spots.

Edinburgh, February 15, 1860.

XLVIII. *Controversy between* Archdeacon Pratt *and* Professor Haughton.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN Archdeacon Pratt's last paper, published in your Number for the present month, he states that the question at issue between him and Professor Haughton is not what I have represented it to be, namely, by what rule the equation

$$\frac{e}{a}\int_0^a \rho' a'^2 - \frac{1}{5a^3}\int_0^a \rho' \frac{d.a'^5 e'}{da'} - \frac{a^2}{5}\int_a^a \rho' \frac{de'}{da'} - \frac{ma^2}{2a^3}\int_0^a \rho' a'^2 = 0 \quad . (12)$$

is to be differentiated when the continuity of the laws of the density and the ellipticity throughout the entire mass is not assumed. Professor Haughton asserts that, by the process of differentiation, and without the assumption, he can deduce the equation

$$\frac{d^2 e}{da^2} + \frac{2\rho a^2}{\int_0^a \rho' a'^2} \frac{de}{da} - \frac{6e}{a^2}\left(1 - \frac{\rho a^3}{3\int_0^a \rho' a'^2}\right) = 0 \quad . . . . . (13)$$

from that just given. Archdeacon Pratt asserts that, without this assumption, the second of these equations does not follow from the first. If this be not a controversy as to the proper mode of differentiating equation (12), I confess myself quite unable to understand what it is. But to obviate all possibility of misconception, I assert, and shall proceed to prove, that equation (13) does follow from equation (12), without any assumption as to the law of density or ellipticity for the solid part of the earth.

I will suppose that, for all values of a' from $a'=0$ to $a'=a$ (in other words, for the whole of the fluid nucleus), we have $e'=\phi(a')$, $\rho'=\psi(a')$, and that, for values beyond a, we have $e'=f(a')$, $\rho'=F(a')$, f and F denoting any functions, continuous or discontinuous.

2 A 2

Let M be the entire mass of the earth. Then since

$$M = 4\pi \int_0^a \rho a'^2,$$

if we introduce this value into the last term of equation (12), multiply by a^3, and substitute for e' and ρ' their values, we shall have

$$a^2\phi(a) \int_0^a a'^2 \psi(a') - \frac{1}{5} \int_0^a \psi(a') \frac{d . a'^5 \phi(a')}{da'} - \frac{a^5}{5} \int_a^a F(a') . f'(a')$$

$$- \frac{mMa^5}{8\pi a^3} = 0.$$

This equation applies to the surface of the fluid nucleus. For the next *surface de niveau* within the fluid the equation will be

$$(a-h)^2 \phi(a-h) \int_0^{a-h} a'^2 \psi(a') - \frac{1}{5} \int_0^{a-h} \psi(a') \frac{d . a'^5 \phi(a')}{da'}$$

$$- \frac{(a-h)^5}{5} \left\{ \int_{a-h}^a \psi(a')\phi'(a') + \int_a^a F(a')f'(a') \right\}^*$$

$$- \frac{mM}{8\pi a^3}(a-h)^5 = 0.$$

Expanding and retaining terms of the first order only, we find

$$- \{2a\phi(a) + a^2\phi'(a)\} \int_0^a a'^2 \psi(a') - a^4 \phi(a)\psi(a) + \frac{1}{5} \psi a \frac{d . a^5\phi(a)}{da}$$

$$+ a^4 \int_a^a F(a')f'(a') - \frac{1}{5} a^5\psi(a)\phi'(a) + \frac{5mMa^4}{8\pi a^3} = 0 ;$$

or striking out the terms which cancel each other, and dividing

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* If I rightly understand Archdeacon Pratt's reasoning, he supposes that, under the conditions stated in the text, the differential coefficient of an expression such as

$$m \int_0^a \rho' + n \int_a^a \rho'$$

with regard to a would be

$$m\psi(a) - nF(a).$$

This, however, is not so. It is easily seen by the mode of reasoning adopted in the text, that this coefficient will be either

$$(m-n)\psi(a) \text{ or } (m-n)F(a),$$

according to the region to which we suppose the differentiation to apply. The result,

$$m\psi(a) - nF(a),$$

could only be obtained by the substitution of $a-h$ for a in the first integral, and of $a+h$ for a in the second, a process which would be of course illogical.

by a^4,

$$-\left(\frac{2\phi a}{a^3} + \frac{\phi' a}{a^2}\right)\int_0^a a'^2\psi(a') + \int_a^a \mathrm{F}(a')f'(a') + \frac{5m\mathrm{M}}{8\pi a^3} = 0.$$

This equation also holds for the surface of separation. For the next *surface de niveau* within the fluid it becomes

$$-\left\{\frac{2\phi(a-h)}{(a-h)^3} + \frac{\phi'(a-h)}{(a-h)^2}\right\}\int_0^{a-h} a'^2\psi a' + \int_{a-h}^a \phi'(a')\psi(a')$$
$$+ \int_a^a \mathrm{F}(a')f'(a') + \frac{5m\mathrm{M}}{a^3} = 0.$$

Expanding as before, and arranging, we find

$$\phi''(a) + \frac{2a^2\psi(a)}{\int_0^a a'^2\psi(a')}\,\phi'(a) - \frac{6\phi(a)}{a^2}\left(1 - \frac{a^3\psi(a)}{3\int_0^a a'^2\psi(a')}\right) = 0,$$

which is identical with equation (13).

The foregoing investigation is of course only an application to the equation (12) of the principles which I stated more generally in your Number for December. I regret that Archdeacon Pratt has obliged me to occupy your pages with a second discussion of a question which in fact belongs to the elements of the integral calculus.

<div align="center">

I am, Gentlemen,

Your obedient Servant,

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Trinity College, Dublin,   JOHN H. JELLETT.

April 1860.

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XLIX. *On the Deposit of Submarine Cables.*

By W. S. B. WOOLHOUSE, *F.R.A.S., F.S.S. &c.*

To the Editors of the Philosophical Magazine and Journal.

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GENTLEMEN,

MY attention has recently been drawn to the dynamical theory of the submergence of telegraphic cables, which has already been discussed both theoretically and practically, at some length and with considerable skill, by Messrs. J. A Longridge and C. H. Brooks, in a valuable paper read before the Institution of Civil Engineers, Feb. 16, 1858. The mathematical theory originally laid down in this excellent paper has since been established, by a different form of process, in an elegant paper by Mr. G. B. Airy, the Astronomer Royal, inserted in your valuable Journal for July 1858. Independently of the practical importance of the subject as an engineering operation, the investigation of the several relations appertaining to it is not devoid of mathematical interest. Indeed I have found the inquiry to be so inviting as to lead me not only to simplify, but to extend the investigations of the general problem somewhat

further than is contained in these papers ; and having, moreover, considered the subject in its more practical bearings, it is presumed that the contribution I am now about to make may be considered of sufficient value for publication.

To avoid confusion we shall, as far as may be practicable, retain the notation and general arrangement of the Astronomer Royal, and for present convenience we shall here briefly state the principal symbols employed, viz.—

n the ship's velocity.

m the velocity of delivery of the cable.

x the horizontal ordinate of a point in the cable curve, measured, from the point where the curve touches the ground, in the direction of the ship's motion.

x' the same, measured from a fixed origin.

y the vertical ordinate of the same point, measured upwards from the bottom.

s the corresponding length of the curve.

ω the inclination of the curve with a horizontal line at the same point.

ρ the radius of curvature.

T the tension, as measured by the length T of cable weighed in water.

g ($=32\cdot19$ feet) the accelerative force of gravity in one second.

g' the same when diminished in the proportion of the cable's real weight to its apparent weight in water.

$a = \dfrac{m^2}{g'}$ twice the height due to the velocity m with diminished gravity.

I. We have first to discuss the problem on the hypothesis that the resistance and friction encountered in passing through the water shall each vary simply as the velocity.

The bottom of the sea is also supposed to be level, and the cable perfectly flexible.

Assume b the coefficient of lateral resistance,

             b' that of longitudinal friction,

$$c = \frac{bn}{g'} = \frac{\text{lateral resistance to velocity } n}{\text{diminished gravity}}.$$

Then, with respect to an element δs of the cable, we shall have

Normal velocity . $= n \sin \omega$ }

Tangential velocity $= m - n \cos \omega$ } downwards,

Normal resistance $= bn \sin \omega$ }

Tangential friction $= b' (m - n \cos \omega)$ } upwards.

Also if the tension were measured by the *real* weight of a length

of cable, we should have that length $=\frac{g'}{g}$ T. Therefore for the accelerative forces on the element δs due to the change of tension, we have

$$\text{Horizontal force} = g\,\frac{\delta}{\delta s}\left(\frac{g'}{g}\,\text{T}\cos\omega\right) = g'\frac{d}{ds}\,(\text{T}\cos\omega),$$

$$\text{Vertical force} \quad = g\,\frac{\delta}{\delta s}\left(\frac{g'}{g}\,\text{T}\sin\omega\right) = g'\frac{d}{ds}\,(\text{T}\sin\omega).$$

By resolving the resistance and friction, the total impressed accelerative forces are therefore,—

$$\text{Horizontal} = g'\frac{d}{ds}\,(\text{T}\cos\omega) - bn\sin^2\omega + b'(m - n\cos\omega)\cos\omega,$$

$$\text{Vertical} \quad = g'\frac{d}{ds}\,(\text{T}\sin\omega) - g' + bn\sin\omega\cos\omega + b'(m - n\cos\omega)\sin\omega.$$

Now if we suppose the movement of the cable to be steady, so that it shall be uniformly deposited at the same speed at which it is delivered, and such that the suspended portion shall retain its form, the absolute velocity of a definite point of the cable will evidently result from a movement down the curve at the velocity of delivery taken in combination with an onward movement of translation equal to the ship's velocity. Thus we have

$$\frac{ds}{dt} = -m, \quad \frac{dx'}{dt} = n - m\cos\omega, \quad \frac{d^2x'}{dt^2} = -m\frac{d\cos\omega}{dt} = m^2\frac{d\cos\omega}{ds},$$

$$\frac{dy}{dt} = -m\sin\omega, \quad \frac{d^2y}{dt^2} = -m\frac{d\sin\omega}{dt} = m^2\frac{d\sin\omega}{ds}.$$

It may be proper to observe that these conditions of steady movement, on which the investigation is made to depend, can only be accurate when the cable is paid out at the same speed as the ship's velocity, or when $m = n$. When this is the case, it is evident that, whatever may be the initial disturbances, the movement will soon cause the cable to assume the permanence of form here supposed. The conditions will, however, approximately subsist when m differs but slightly from n, as the form of the curve will then only be subject to a very gradual change. But if m should differ much from n, we ought not to place much reliance in the accuracy of the results[*].

Equating the values of $\frac{d^2x'}{dt^2}$, $\frac{d^2y}{dt^2}$ with the impressed forces,

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[*] The discussion of the problem, taken in all its generality, supposing the form as well as the position of the curve to vary, would lead to expressions too complicated to be of any practical utility. Besides, this is not needed, as all irregular movement should be avoided during the operation.

we have

$$m^2 \frac{d\cos\omega}{ds} = g'\frac{d}{ds}(\mathrm{T}\cos\omega) - bn\sin^2\omega + b'(m - n\cos\omega)\cos\omega,$$

$$m^2 \frac{d\sin\omega}{ds} = g'\frac{d}{ds}(\mathrm{T}\sin\omega) - g' + bn\sin\omega\cos\omega + b'(m - n\cos\omega)\sin\omega;$$

or, after transposing and dividing by g',

$$\left. \begin{aligned} 0 &= \frac{d}{ds}\{(\mathrm{T}-a)\cos\omega\} - e\sin^2\omega + \frac{b'}{b}\left(\frac{m}{n} - \cos\omega\right)\cos\omega \\ 0 &= \frac{d}{ds}\{(\mathrm{T}-a)\sin\omega\} - 1 + e\sin\omega\cos\omega + \frac{b'}{b}e\left(\frac{m}{n} - \cos\omega\right)\sin\omega \end{aligned} \right\} . (1)$$

Multiply these respectively by $\cos\omega$, $\sin\omega$, and add; and next multiply them by $\sin\omega$, $\cos\omega$, and subtract; then

$$\left. \begin{aligned} \frac{d\mathrm{T}}{ds} &= \sin\omega - \frac{b'}{b}e\left(\frac{m}{n} - \cos\omega\right) \\ (\mathrm{T}-a)\frac{d\omega}{ds} &= \cos\omega - e\sin\omega \end{aligned} \right\} . \quad . \quad . \quad . \quad (2)$$

The latter of these equations gives

$$\mathrm{T}-a = \rho(\cos\omega - e\sin\omega); \quad . \quad . \quad . \quad . \quad (3)$$

and at the lowest point where $\omega = 0$, $\mathrm{T}_0 - a = \rho_0$.

From the equations (2) we also obtain

$$\frac{d\mathrm{T}}{\mathrm{T}-a} = d\omega\frac{\sin\omega - \dfrac{b'}{b}e\left(\dfrac{m}{n} - \cos\omega\right)}{\cos\omega - e\sin\omega}. \quad . \quad . \quad (4)$$

To integrate this equation, put $e = \cot\lambda$; then λ is evidently the limiting angle or maximum value of ω, and

$$\cos\omega - e\sin\omega = \frac{\sin(\lambda - \omega)}{\sin\lambda},$$

$$\sin\omega = \sin\lambda\cos(\lambda - \omega) - \cos\lambda\sin(\lambda - \omega),$$

$$\cos\omega = \cos\lambda\cos(\lambda - \omega) + \sin\lambda\sin(\lambda - \omega).$$

Substituting these values in (4) and integrating, remembering that $\mathrm{T}-a = \rho_0$ when $\omega = 0$, we get

$$\begin{aligned} \log\frac{\mathrm{T}-a}{\rho_0} &= \sin^2\lambda\log\frac{\sin\lambda}{\sin(\lambda - \omega)} - \omega\cos\lambda\sin\lambda \\ &\quad + \frac{b'}{b}e\left\{\cos\lambda\sin\lambda\log\frac{\sin\lambda}{\sin(\lambda - \omega)} + \omega\sin^2\lambda - \frac{m}{n}\sin\lambda\log\frac{\tan\frac{1}{2}\lambda}{\tan\frac{1}{2}(\lambda - \omega)}\right\} \\ &= \left(\sin^2\lambda + \frac{b'}{b}\cos^2\lambda\right)\log\frac{\sin\lambda}{\sin(\lambda - \omega)} - \frac{b'}{b}\frac{m}{n}\cos\lambda\log\frac{\tan\frac{1}{2}\lambda}{\tan\frac{1}{2}(\lambda - \omega)} \\ &\quad - \omega\cos\lambda\sin\lambda\left(1 - \frac{b'}{b}\right). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5) \end{aligned}$$

Again, substituting the value of $T - a$ given by (3), we get

$$\log \frac{\rho}{\rho_0} = \left\{ 2 - \left(1 - \frac{b'}{b} \right) \cos^2 \lambda \right\} \log \frac{\sin \lambda}{\sin (\lambda - \omega)}$$
$$- \frac{b'}{b} \frac{m}{n} \cos \lambda \log \frac{\tan \frac{1}{2} \lambda}{\tan \frac{1}{2} (\lambda - \omega)} - \left(1 - \frac{b'}{b} \right) \omega \cos \lambda \sin \lambda, \quad (6)$$

which is an equation of the cable curve, determining the proportionate radius of curvature in terms of the angle ω.

Also since

$$\frac{s}{\rho_0} = \int \frac{\rho}{\rho_0} d\omega, \quad \frac{x}{\rho_0} = \int \frac{\rho}{\rho_0} d\omega \cos \omega, \quad \frac{y}{\rho_0} = \int \frac{\rho}{\rho_0} d\omega \sin \omega,$$

these are functions of ω alone; and the constant ρ_0, on which the absolute magnitudes depend, may be found by comparing a calculated value of $\frac{y}{\rho_0}$ with the known depth of the sea.

By integrating the first of equations (2) we have likewise the following relation,

$$T - a = \rho_0 + y - \frac{b'}{b} e \left(\frac{m}{n} s - x \right). \quad \quad (7)$$

The foregoing equations, which are general, become much simplified if we assume, as Mr. Airy has done, that $b' = b$ and $m = n$. Thus if we put

$$z = \left\{ \frac{\tan \frac{1}{2} \lambda}{\tan \frac{1}{2} (\lambda - \omega)} \right\}^{-\cos \lambda}, \quad \quad (8)$$

the values of which are evidently comprised between 0 and 1, equation (5) gives

$$\frac{T - a}{\rho_0} = \frac{\sin \lambda}{\sin (\lambda - \omega)} \cdot z; \quad \quad (9)$$

and equation (7) becomes

$$T - a = \rho_0 + y - e(s - x). \quad \quad (10)$$

Equations (1) also become

$$0 = \frac{d}{ds} \{ (T - a) \cos \omega \} - e(1 - \cos \omega),$$

$$0 = \frac{d}{ds} \{ (T - a) \sin \omega \} - 1 + e \sin \omega,$$

and, by immediate integration, give

$$\left. \begin{array}{l} (T - a) \cos \omega = \rho_0 + e(s - x) \\ (T - a) \sin \omega = s - ey \end{array} \right\} \quad \quad (11)$$

The first of these and (10) give

$$\left.\begin{array}{l}(T-a)(1+\cos\omega)=2\rho_0+y\\(T-a)(1-\cos\omega)=y-2e(s-x)\end{array}\right\}\quad.\quad(12)$$

And from (9), (11) we deduce

$$z=\frac{T-a}{\rho_0}\cdot\frac{\sin(\lambda-\omega)}{\sin\lambda}=\frac{T-a}{\rho_0}(\cos\omega-e\sin\omega)$$

$$=1-e\frac{x}{\rho_0}+e^2\frac{y}{\rho_0}.$$

The coordinates and length of the curve, as compared with ρ_0, the radius of curvature at the lowest point, are hence determined by the simple formulæ

$$\left.\begin{array}{l}\tan\frac{1}{2}(\lambda-\omega)=\tan\frac{1}{2}\lambda\cdot z^{\sec\lambda},\\[2mm]\dfrac{T-a}{\rho_0}=\dfrac{\sin\lambda}{\sin(\lambda-\omega)}\cdot z,\\[2mm]\dfrac{y}{\rho_0}=\dfrac{T-a}{\rho_0}(1+\cos\omega)-2,\\[2mm]\dfrac{x}{\rho_0}=e\dfrac{y}{\rho_0}+\dfrac{1-z}{e},\\[2mm]\dfrac{s}{\rho_0}=e\dfrac{y}{\rho_0}+\dfrac{T-a}{\rho_0}\sin\omega.\end{array}\right\}\quad.\quad.\quad.\quad(13)$$

The Tables given by the Astronomer Royal can be constructed with the greatest possible facility from these formulæ; and if the first of them be replaced by equation (8), the calculations may be performed for given values of ω.

For any integral value of e, up to 10, the constants may be taken from the following Table:—

e.	λ.	$\cos\lambda$.	$\sec\lambda$.	$\log\tan\frac{1}{2}\lambda$.	$\log\sin\lambda$.
1	45° 0·0	0·70711	1·41420	9·61722	9·84949
2	26 33·9	0·89444	1·11802	9·37303	9·65052
3	18 26·1	0·94868	1·05410	9·21026	9·50000
4	14 2·2	0·97015	1·03077	9·09027	9·38478
5	11 18·6	0·98057	1·01981	8·99572	9·29251
6	9 27·7	0·98639	1·01379	8·91783	9·21590
7	8 7·8	0·98994	1·01016	8·85167	9·15051
8	7 7·5	0·99227	1·00779	8·79419	9·09354
9	6 20·4	0·99389	1·00614	8·74340	9·04309
10	5 42·6	0·99504	1·00499	8·69789	8·99784

At any time during the actual operation of laying the cable, it is evident that the ship's velocity, the depth of the sea, and the tension and inclination of the cable at the ship, can be ascertained by observation. With deep water the inclination ω will

not differ much from the limiting angle λ; and as a small error in ω would then considerably affect the other values, it will be preferable to determine this angle by calculation. For this purpose we have

$$\frac{y}{T-a} = 1 + \cos \omega - 2\frac{\sin (\lambda - \omega)}{\sin \lambda}\left\{ \frac{\tan \frac{1}{2}\lambda}{\tan \frac{1}{2}(\lambda - \omega)} \right\}^{\cos \lambda}. \quad (14)$$

If for given values of e or λ, values of this expression be calculated and tabulated under ω, then by entering this Table with the known values of $\frac{y}{T-a}$, the angles ω will be readily deduced.

From (12),

$$\tan^2 \tfrac{1}{2}\omega = \frac{y - 2e(s-x)}{y + 2\rho_0};$$

$$\therefore 2\rho_0 = \frac{y - 2e(s-x)}{\tan^2 \tfrac{1}{2}\omega} - y.$$

Hence we conclude that when, by extra paying out, the amount of "slack" or "stray length" $(s-x)$ is increased, and the inclination ω also increased, the radius (ρ_0) of curvature at the lowest point of the curve becomes sensibly diminished; and it will be evanescent when

$$2e\frac{s-x}{y} = 1 - \tan^2 \tfrac{1}{2}\omega.$$

The value of the constant ρ_0 will perhaps be best determined by the formula

$$\rho_0 = \frac{(T-a)(1 + \cos \omega) - y}{2}; \quad \cdots \quad (15)$$

and the amount of stray length by the formula

$$s - x = \frac{(T-a)\cos \omega - \rho_0}{e}$$

$$= \frac{y - (T-a)(1 - \cos \omega)}{2 \cot \lambda}, \quad \cdots \quad (16)$$

which are deduced from (11) and (12). For the calculation of these the observed value of ω will probably be sufficiently accurate.

We have only further to remark that the curve has a rectilinear asymptote inclined at the limiting angle λ with the horizontal; that the horizontal distance of this asymptote from the lowest point $= \frac{\rho_0}{e}$, and the horizontal distance from any other point of the curve = this distance multiplied by z. Also if from any point in the curve a tangent be drawn terminating in

the asymptote, e times the length of this tangent will be equal to $T-a$.

II. We propose now to renew the investigation, assuming the resistance and friction to each vary as the square of the velocity, which supposition is more nearly in accordance with the actual resistances as determined by experiment.   Let

B be the coefficient of lateral resistance,
B′ that of longitudinal friction,

$$e^2 = \frac{Bn^2}{g'} = \frac{\text{lateral resistance to velocity } n}{\text{diminished gravity}}.$$

Then with respect to an element δs of the cable, we shall have

$$\text{Normal resistance} = Bn^2 \sin^2 \omega,$$
$$\text{Tangential friction} = B'(m - n \cos \omega)^2 ;$$

and, proceeding as before, we obtain

$$m^2 \frac{d \cos \omega}{ds} = g' \frac{d}{ds}(T \cos \omega) - Bn^2 \sin^3 \omega + B'(m - n \cos \omega)^2 \cos \omega,$$

$$m^2 \frac{d \sin \omega}{ds} = g' \frac{d}{ds}(T \sin \omega) - g' + Bn^2 \sin^2 \omega \cos \omega + B'(m - n \cos \omega)^2 \sin \omega ;$$

or, transposing and dividing by g',

$$\left. \begin{aligned}
0 &= \frac{d}{ds}\{(T-a)\cos \omega\} - e^2 \sin^3 \omega + \frac{B'}{B} e^2 \left(\frac{m}{n} - \cos \omega\right)^2 \cos \omega, \\
0 &= \frac{d}{ds}\{(T-a)\sin \omega\} - 1 + e^2 \sin^2 \omega \cos \omega + \frac{B'}{B} e^2 \left(\frac{m}{n} - \cos \omega\right)^2 \sin \omega,
\end{aligned} \right\} \quad (1)$$

which, in substance, are the equations finally arrived at by Mr. Airy.

Multiply equations (1) respectively by $\cos \omega$, $\sin \omega$, and add; and afterwards multiply them by $\sin \omega$, $\cos \omega$, and subtract; then

$$\left. \begin{aligned}
\frac{dT}{ds} &= \sin \omega - \frac{B'}{B} e^2 \left(\frac{m}{n} - \cos \omega\right)^2 \\
(T-a) \frac{d\omega}{ds} &= \cos \omega - e^2 \sin^2 \omega,
\end{aligned} \right\} \quad . \quad . \quad . \quad (2)$$

which agree with the equations obtained by Messrs. Longridge and Brooks*.

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* In the investigation of Problem III., Messrs. Longridge and Brooks have disregarded the effective forces as inconsiderable. To supply these, we have, at the point C, the horizontal velocity $= \nu(1 - \cos \alpha)$, and the vertical velocity $= -\nu \sin \alpha$. Now with any variable velocity V, the effective accelerative force $= \dfrac{dV}{dt} = -\nu \dfrac{dV}{ds}$; and multiplying by $\dfrac{1}{g'} ds$ and in-

From the latter of these equations,

$$T - a = \rho \left(\cos \omega - e^2 \sin^2 \omega \right) ; \quad \ldots \quad (3)$$

and at the lowest point where $\omega = 0$, $T_0 - a = \rho_0$. Equations (2) also give by division,

$$\frac{dT}{T - a} = d\omega \frac{\sin \omega - \frac{B'}{B} e^2 \left(\frac{m}{n} - \cos \omega \right)^2}{\cos \omega - e^2 \sin^2 \omega}. \quad \ldots \quad (4)$$

To facilitate the integration of this expression, let λ be the limiting angle of the curve or value of ω which makes

$$\cos \omega - e^2 \sin^2 \omega = 0.$$

Then

$$0 = \cos \lambda - e^2 \sin^2 \lambda = e^2 \cos^2 \lambda + \cos \lambda - e^2 ;$$

and if for brevity we put $\cos \lambda = \alpha$, α and $-\dfrac{1}{\alpha}$ will be the roots of the quadratic,

$$\alpha^2 + \frac{\alpha}{e^2} - 1 = 0. \quad \ldots \quad \ldots \quad (5)$$

Therefore

$$\frac{1}{\cos \omega - e^2 \sin^2 \omega} = \frac{1}{e^2 \left(\cos \omega - \alpha \right) \left(\cos \omega + \dfrac{1}{\alpha} \right)}$$

$$= \frac{1 - \alpha^2}{1 + \alpha^2} \left(\frac{1}{\cos \omega - \alpha} - \frac{1}{\cos \omega + \dfrac{1}{\alpha}} \right).$$

By substitution and integration,

$$\log \frac{T - a}{c} = \frac{1 - \alpha^2}{1 + \alpha^2} \log \frac{\cos \omega + \dfrac{1}{\alpha}}{\cos \omega - \alpha}$$

$$- \frac{B'}{B} \cdot \frac{\alpha}{1 + \alpha^2} \int d\omega \left\{ \frac{\left(\dfrac{m}{n} - \cos \omega \right)^2}{\cos \omega - \alpha} - \frac{\left(\dfrac{m}{n} - \cos \omega \right)^2}{\cos \omega + \dfrac{1}{\alpha}} \right\}.$$

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tegrating, the accumulated force, as measured by weight in water, $= -\dfrac{\nu}{g'} V.$ Therefore the vertical effective force $= \dfrac{\nu^2}{g'} \sin A$, and the horizontal force $= -\dfrac{\nu^2}{g'} (1 - \cos A)$. If the expressions (1) and (2), instead of being put equal to zero, be respectively equated with these, the only effect on the equations (3), (4) which result from them, will be the substitution of $t - \dfrac{\nu^2}{g'}$ for t in the latter, and they are then identical with our equations (2).

The correction $\dfrac{\nu^2}{g'}$, or a, is always a very small quantity.

But

$$\frac{\left(\dfrac{m}{n}-\cos\omega\right)^2}{\cos\omega-\alpha}=\cos\omega-\left(\frac{2m}{n}-\alpha\right)+\frac{\left(\dfrac{m}{n}-\alpha\right)^2}{\cos\omega-\alpha},$$

$$\frac{\left(\dfrac{m}{n}-\cos\omega\right)^2}{\cos\omega+\dfrac{1}{\alpha}}=\cos\omega-\left(\frac{2m}{n}+\frac{1}{\alpha}\right)+\frac{\left(\dfrac{m}{n}+\dfrac{1}{\alpha}\right)^2}{\cos\omega+\dfrac{1}{\alpha}},$$

$$\therefore \log\frac{\mathrm{T}-a}{c}=\frac{1-\alpha^2}{1+\alpha^2}\log\frac{\cos\omega+\dfrac{1}{\alpha}}{\cos\omega-\alpha}$$

$$-\frac{\mathrm{B'}}{\mathrm{B}}\cdot\frac{\alpha}{1+\alpha^2}\int d\omega\left\{\frac{1}{\alpha}+\alpha+\frac{\left(\dfrac{m}{n}-\alpha\right)^2}{\cos\omega-\alpha}-\frac{\left(\dfrac{m}{n}+\dfrac{1}{\alpha}\right)^2}{\cos\omega+\dfrac{1}{\alpha}}\right\},$$

which, finally integrated and corrected so as to make $\mathrm{T}_0-a=\rho_0$ when $\omega=0$, gives

$$\log\frac{\mathrm{T}-a}{d_0}=\frac{1-\alpha^2}{1+\alpha^2}\log\left(\frac{1-\alpha}{1+\alpha}\cdot\frac{\alpha\cos\omega+1}{\cos\omega-\alpha}\right)$$

$$-\frac{\mathrm{B'}}{\mathrm{B}}\left\{\omega+\frac{\alpha\left(\dfrac{m}{n}-\alpha\right)^2}{(1+\alpha^2)\sqrt{1-\alpha^2}}\log\frac{1-\alpha\cos\omega+\sin\omega\sqrt{1-\alpha^2}}{\cos\omega-\alpha}\right.$$

$$\left.-\frac{\left(\dfrac{m}{n}\alpha+1\right)^2}{(1+\alpha^2)\sqrt{1-\alpha^2}}\sin^{-1}\frac{\sin\omega\sqrt{1-\alpha^2}}{\alpha\cos\omega+1}\right\}.\ (6)$$

If the value of $\mathrm{T}-a$ by (3) be substituted, we shall obtain an equation of the curve exhibiting the radius of curvature at any point as a function of ω.

To adapt the expressions to numerical calculation, assume

$$2e^2=\tan 2\mu,\quad \cos\lambda=\tan\mu,\quad \cos\omega=\tan\theta;\ .\ .\ (7)$$

then

$$\frac{1}{\alpha}+\alpha=2\ \mathrm{cosec}\ 2\mu,\quad \frac{1}{\alpha}-\alpha=2\cot 2\mu,\quad \frac{1-\alpha^2}{1+\alpha^2}=\cos 2\mu,$$

and

$$\frac{1-\alpha}{1+\alpha}=\tan^2\tfrac{1}{2}\lambda.$$

Therefore

$$\frac{1-\alpha}{1+\alpha} \cdot \frac{\alpha \cos \omega + 1}{\cos \omega - \alpha} = \tan^2 \tfrac{1}{2}\lambda \frac{\tan \mu \tan \theta + 1}{\tan \theta - \tan \mu} = \frac{\tan^2 \tfrac{1}{2}\lambda}{\tan(\theta - \mu)},$$

$$\frac{1 - \alpha \cos \omega + \sin \omega \sqrt{1 - \alpha^2}}{\cos \omega - \alpha} = \frac{1 - \cos(\lambda + \omega)}{\cos \omega - \cos \lambda}$$

$$= \frac{2 \sin^2 \tfrac{1}{2}(\lambda + \omega)}{2 \sin \tfrac{1}{2}(\lambda + \omega) \sin \tfrac{1}{2}(\lambda - \omega)} = \frac{\sin \tfrac{1}{2}(\lambda + \omega)}{\sin \tfrac{1}{2}(\lambda - \omega)}.$$

Again, if $\sin \psi = \dfrac{\sin \omega \sqrt{1 - \alpha^2}}{\alpha \cos \omega + 1}$; then $\cos \psi = \dfrac{\cos \omega + \alpha}{\alpha \cos \omega + 1}$, and

$$\tan^2 \tfrac{1}{2}\psi = \frac{1 - \cos \psi}{1 + \cos \psi} = \frac{(1 - \alpha)(1 - \cos \omega)}{(1 + \alpha)(1 + \cos \omega)} = \tan^2 \tfrac{1}{2}\lambda \tan^2 \tfrac{1}{2}\omega.$$

Hence

$$\log \frac{T - a}{\rho_0} = \cos 2\mu \log \frac{\tan^2 \tfrac{1}{2}\lambda}{\tan(\theta - \mu)} - \frac{B'}{B} \Bigg\{ \omega - \frac{\cos^2 \mu}{\tan \lambda}\left(\frac{m}{n} - \alpha\right)^2$$

$$\log \frac{\sin \tfrac{1}{2}(\lambda + \omega)}{\sin \tfrac{1}{2}(\lambda - \omega)} - \frac{2 \sin^2 \mu}{\sin \lambda}\left(\frac{m}{n} + \frac{1}{\alpha}\right)^2 \tan^{-1}\left(\tan \tfrac{1}{2}\lambda \tan \tfrac{1}{2}\omega\right) \Bigg\}.$$

Or, if

$$c_1 = \frac{1 - \alpha^2}{1 + \alpha^2} = \cos 2\mu, \quad c_2 = \frac{1 - \alpha}{1 + \alpha} = \tan^2 \tfrac{1}{2}\lambda, \quad c_3 = \log \epsilon \times \text{arc of } 1°,$$

$$c_4 = \frac{\alpha \left(\frac{m}{n} - \alpha\right)^2}{(1 + \alpha^2)\sqrt{1 - \alpha^2}} = \frac{\cos^2 \mu}{\tan \lambda}\left(\frac{m}{n} - \alpha\right)^2, \quad c_5 = \sqrt{\frac{1 - \alpha}{1 + \alpha}} = \tan \tfrac{1}{2}\lambda,$$

$$c_6 = 2c_3 \frac{\left(1 + \frac{m}{n}\alpha\right)^2}{(1 + \alpha^2)\sqrt{1 - \alpha^2}} = 2c_3 \frac{\cos^2 \mu}{\sin \lambda}\left(1 + \frac{m}{n}\alpha\right)^2;$$

then

$$\log \frac{T - a}{\rho_0} = c_1 \log \frac{c_2}{\tan(\theta - \mu)} - \frac{B'}{B} \Bigg\{ c_3 \omega + c_4 \log \frac{\sin \tfrac{1}{2}(\lambda + \omega)}{\sin \tfrac{1}{2}(\lambda - \omega)}$$

$$- c_6 \tan^{-1}\left(c_5 \tan \tfrac{1}{2}\omega\right) \Bigg\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

in which ω is expressed in degrees, and the logarithms are now adapted to the system of Briggs as commonly used.

The logarithmic values of the constants, including the angle μ and the limiting angle λ, are given in the following Table for integral values of e, and supposing that $m = n$*.

* When the ship is stationary, or $e = 0$, the curve is the common catenary, and $\lambda = 90°$, $\alpha = 0$; $\therefore$ by (6) $\dfrac{T - a}{\rho_0} = \sec \omega$, and by (3) $\dfrac{T - a}{\rho} = \cos \omega$;

$$\log c_3 = 7\cdot 87966.$$

e.	λ.	μ.	$\log c_1$.	$\log c_2$.	$\log c_4$.	$\log c_5$.	$\log c_6$.
0	90 0·0	0 0·0	0·00000	0·00000	$-\infty$	0·00000	8·18069
1	51 49·6	31 43·0	9·65052	9·37304	8·91908	9·68652	8·56267
2	28 1·3	41 26·2	9·09355	8·79424	8·16171	9·39712	8·80811
3	18 55·2	43 24·6	8·74406	8·44348	7·65260	9·22174	8·97051
4	14 15·0	44 6·3	8·49464	8·19382	7·28361	9·09691	9·09040
5	11 25·0	44 25·6	8·30094	7·99965	6·99514	8·99983	9·18513
6	9 31·0	44 36·1	8·14263	7·84067	6·75820	8·92034	9·26335
7	8 11·0	44 42·5	8·00875	7·70903	6·56034	8·85452	9·32837
8	7 9·0	44 46·6	7·89278	7·59143	6·38534	8·79572	9·38670
9	6 21·5	44 49·4	7·79047	7·48928	6·23266	8·74464	9·43738
10	5 43·0	44 51·4	7·69896	7·39670	6·09583	8·69835	9·48342

For brevity put $k = \dfrac{T-a}{\rho_0}$ and

$$h = \int \frac{d\omega \sin \omega}{\cos \omega - e^2 \sin^2 \omega} = \frac{1-\alpha^2}{1+\alpha^2} \log \left(\frac{1-\alpha}{1+\alpha} \cdot \frac{\alpha \cos \omega + 1}{\cos \omega - \alpha} \right)$$

$$= c_1 \log \frac{c_2}{\tan (\theta - \mu)},$$

or

$$\tan (\theta - \mu) = c_2 \epsilon^{-\frac{h}{c_1}}. \quad . \quad . \quad . \quad . \quad . \quad (9)$$

Then employing h as an independent auxiliary variable, we shall have

$$\frac{1}{\rho_0} \cdot \frac{ds}{dh} = \frac{\rho}{\rho_0} \cdot \frac{d\omega}{dh} = \frac{\rho}{\rho_0} \cdot \frac{\cos \omega - e^2 \sin^2 \omega}{\sin \omega} = \frac{T-a}{\rho_0 \sin \omega} = \frac{k}{\sin \omega};$$

$$\therefore \frac{1}{\rho_0} \cdot \frac{dy}{dh} = k.$$

Hence by assuming a uniform succession of values of h and calculating values of θ from (9), and thence values of ω from (7) and k from (8), the corresponding values of $\dfrac{y}{\rho_0} = \int k \, dh$ can be obtained by the method of quadratures. Thus, if the values of k be differenced twice, and (k) denote the arithmetical mean

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$$\therefore \frac{\rho}{\rho_0} = \sec^2 \omega. \text{ Hence also in this case,}$$

$$\frac{s}{\rho_0} = \int \frac{\rho \, d\omega}{\rho_0} = \int d\omega \sec^2 \omega = \tan \omega.$$

$$\frac{x}{\rho_0} = \int \frac{\rho \, d\omega}{\rho_0} \cos \omega = \int d\omega \sec \omega = \log \tan (45^\circ + \tfrac{1}{2} \omega).$$

$$\frac{y}{\rho_0} \doteq \int \frac{\rho \, d\omega}{\rho_0} = \int d\omega \sec \omega \tan \omega = \sec \omega - 1.$$

of two consecutive values k, $k + \Delta k$, and (Δ_2) the mean of the second differences which stand respectively opposite to them, the increment of $\frac{y}{\rho_0}$ may be calculated from the formula

$$\Delta \left(\frac{y}{\rho_0} \right) = (k) - \frac{1}{6} (\Delta_2).$$

For the computation of $\frac{x}{\rho_0}$ we have

$$\frac{1}{\rho_0} \cdot \frac{dx}{dh} = \frac{1}{\rho_0} \cdot \frac{ds}{dh} \cos \omega = k \cot \omega \; ;$$

but as the value of this quantity is indefinitely great when ω is indefinitely small, the integration by the same method becomes impracticable. To obviate this inconvenience, calculate a table of the values of

$$Q = \frac{1}{\rho_0} \cdot \frac{dx}{dh} \sqrt{h} = k \sqrt{h} \cot \omega,$$

which will not be subject to any abrupt change. Then $\frac{x}{\rho_0}$ will be the integral of $Q \frac{dh}{\sqrt{h}}$.

The value of Q at the lowest point of the curve, where $\omega = 0$ and $h = 0$, may be found thus:—Since $k_0 = 1$, we shall have in the immediate vicinity of this point $\frac{y}{\rho_0} = \int dh = h$. Also since an element of the curve will coincide with the circle of curvature, we shall also have $x = \sqrt{y(2\rho_0 - y)}$; or substituting $y = \rho_0 h$,

$$\frac{x}{\rho_0} = \sqrt{h(2-h)} \; ; \; \therefore \; \frac{1}{\rho_0} \cdot \frac{dx}{dh} \sqrt{h} = \sqrt{\frac{1-h}{2-h}} = \tfrac{1}{2} \sqrt{2} \text{ when } h = 0.$$

Now the quantities Q being differenced, and Δ_1, Δ_2, &c. denoting the differences which immediately succeed a given value of h, and representing the values of h by ordinal numbers q, so that $q = \frac{h}{\Delta h}$, the value of Q when h becomes $h + i \Delta h$, or when q becomes $q + i$, will be $Q + i \Delta_1 + \frac{i(i-1)}{2} \Delta_2 + \&c.$; and the increment of $\frac{x}{\rho_0}$ in passing from h to $h + \Delta h$, or from q to $q + 1$, will be

$$\Delta \frac{x}{\rho_0} = \sqrt{\Delta h} \int_0^1 \frac{di}{\sqrt{q+i}} \left\{ Q + i\Delta_1 + \frac{i(i-1)}{2} \Delta_2, \&c. \right\}^*.$$

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* For the integration of the terms of this expression I have arrived at the following curious general form :—

$$\int \frac{di}{\sqrt{q+i}} \cdot i^n = 2 \cdot \frac{2 . 4 \ldots 2n}{3 . 5 \ldots 2n+1} (q+i)^{\frac{1}{2}} \left\{ q^n \left(1 + \frac{i}{q} \right)^{-\frac{1}{2}} \text{expanded as far as } i^n \right\}.$$

Also, if for brevity

$$\int_0^1 \frac{di}{\sqrt{q+i}} = (0), \quad \int_0^1 \frac{di}{\sqrt{q+i}}.i = (1), \quad \int_0^1 \frac{di}{\sqrt{q+i}}.i^2 = (2),$$

$$\int_0^1 \frac{di}{\sqrt{q+i}}.i^3 = (3), \&c.,$$

the values of these integrals may be deduced from the formulæ

$$(0) = 2\{\sqrt{q+1} - \sqrt{q}\},$$
$$(1) = \tfrac{2}{3}\{\sqrt{q+1} - q(0)\},$$
$$(2) = \tfrac{2}{5}\{\sqrt{q+1} - 2q(1)\},$$
$$(3) = \tfrac{2}{7}\{\sqrt{q+1} - 3q(2)\},$$

&c.   &c.

Hence the coefficients of Q, Δ_1, Δ_2, &c. in the value of $\Delta \dfrac{x}{\rho_0}$ may be found with the use of a Table of square roots. Those of the first two will be sufficient for our present purpose, provided that Δ_1 be diminished in each case by one-sixth of the mean value of Δ_2; and the values up to $q = 30$ are exhibited in the following Table:—

Coefficients for integrating $Q\dfrac{dq}{\sqrt{q}}$.

q.	Coeff. of Q.	Coeff. of $\Delta_1 - \tfrac{1}{6}(\Delta_2)$.	q.	Coeff. of Q.	Coeff. of $\Delta_1 - \tfrac{1}{6}(\Delta_2)$.
0	2·00000	0·6667	16	0·24621	0·1225
1	0·82843	0·3905	17	0·23907	0·1190
2	0·63567	0·3071	18	0·23252	0·1157
3	0·53590	0·2615	19	0·22647	0·1128
4	0·47214	0·2317	20	0·22088	0·1100
5	0·42684	0·2102	21	0·21568	0·1074
6	0·39252	0·1937	22	0·21083	0·1050
7	0·36535	0·1806	23	0·20630	0·1028
8	0·34315	0·1699	24	0·20204	0·1007
9	0·32456	0·1609	25	0·19804	0·0987
10	0·30869	0·1531	26	0·19427	0·0968
11	0·29495	0·1464	27	0·19070	0·0951
12	0·28290	0·1405	28	0·18732	0·0934
13	0·27221	0·1353	29	0·18412	0·0918
14	0·26265	0·1306	30	0·18108	0·0903
15	0·25403	0·1263			

The value of the integral might be otherwise found by putting it under the form $2\int Q d(\sqrt{h})$, and considering $\sqrt{h}$ as the auxiliary variable.

If we examine the foregoing equations, it will appear that the longitudinal friction which introduces the terms containing

B′, has so little influence on the *form* of the curve that its effects may be practically disregarded, and their omission will greatly simplify the formulæ. The differential equation (4), from which we have obtained the formula (8) for determining the tension, obviously indicates, when $m = n$, that by neglecting the longitudinal friction the value of the tension will be sensibly increased only towards the upper extremity of the cable, and even there but slightly, as the coefficient B′ is small as compared with B. Therefore by (3) the radii of curvature towards this extremity will also be slightly increased in the same proportion. But as this portion of the curve is nearly straight, it is evident that a small proportionate augmentation of the large radii of curvature cannot produce any sensible divergence throughout the limited extent of the curve we have under consideration. For a given value of ω the coordinates $x = \int \rho \, d\omega \cos \omega$, $y = \int \rho \, d\omega \sin \omega$ will be *both* slightly increased; but the point may be considered to be merely transferred along the curve, as the divergence towards the convex side will be extremely minute. We shall therefore deduce the formulæ assuming B′ = 0, which will be more convenient, and sufficiently accurate for all practical purposes.

Making B′ = 0 in (6), we get

$$\frac{T-a}{\rho_0} = \left(\frac{1-\alpha}{1+\alpha} \cdot \frac{\alpha \cos \omega + 1}{\cos \omega - \alpha}\right)^{\frac{1-\alpha^2}{1+\alpha^2}} = \left\{\frac{\tan^2 \frac{1}{2}\mu}{\tan(\theta - \mu)}\right\}^{\cos 2\mu}, \quad (10)$$

Hence if T_1, ω_1 denote values referring to *any* given point, we shall have

$$\frac{T-a}{T_1-a} = \left(\frac{\alpha \cos \omega + 1}{\alpha \cos \omega_1 + 1} \cdot \frac{\cos \omega_1 - \alpha}{\cos \omega - \alpha}\right)^{\frac{1-\alpha^2}{1+\alpha^2}} = \left\{\frac{\cot(\theta - \mu)}{\cot(\theta_1 - \mu)}\right\}^{c_1}. \quad (11)$$

Again, since $\cos \omega - e^2 \sin^2 \omega = e^2 (\cos \omega - \alpha)\left(\cos \omega + \frac{1}{\alpha}\right)$, from (3) we get

$$\frac{T-a}{T_1-a} = \frac{\rho}{\rho_1} \cdot \frac{\cos \omega - \alpha}{\cos \omega_1 - \alpha} \cdot \frac{\alpha \cos \omega + 1}{\alpha \cos \omega_1 + 1};$$

$$\therefore \frac{\rho}{\rho_1} = \left(\frac{\cos \omega - \alpha}{\cos \omega_1 - \alpha}\right)^{-\frac{2}{1+\alpha^2}} \left(\frac{\alpha \cos \omega + 1}{\alpha \cos \omega_1 + 1}\right)^{-\frac{2\alpha}{1+\alpha^2}}. \quad (12)$$

Also the first of the equations (2) gives, by integration,

$$T - a = \rho_0 + y; \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (13)$$

$$\therefore \frac{y}{\rho_0} = \left(\frac{1-\alpha}{1+\alpha} \cdot \frac{\alpha \cos \omega + 1}{\cos \omega - \alpha}\right)^{\frac{1-\alpha}{1+\alpha^2}} - 1, \quad \cdot \quad \cdot \quad (14)$$

which is the equation of the curve. Or, adopting the former constants,

$$\frac{y}{\rho_0} = \left\{\frac{c_2}{\tan(\theta - \mu)}\right\}^{c_1} - 1. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (15)$$

The values of $\frac{x}{\rho_0}$, $\frac{s}{\rho_0}$, if required, must be found, as before, by the method of quadratures. As an example, I have taken the case of $e=2$, $B'=\frac{1}{4}B$, $\rho_0=1$, and carefully calculated the coordinates, &c. from the general formulæ; and the results are given in the following Table:—

Table for $e=2$, $B'=\frac{1}{4}B$, $\rho_0=1$.

h.	q.	ω.	$Q=\dfrac{dx}{dh}\sqrt{h}$.	$k=\dfrac{dy}{dh}$ $=\text{T}-a$.	x.	y.
·00	0	0° 0·0′	0·70711	1·00000	0·00000	0·00000
·01	1	7 55·0	0·72557	1·01004	0·14265	0·01005
·02	2	10 58·3	0·74418	1·02021	0·20349	0·02020
·03	3	13 9·7	0·76294	1·03043	0·25137	0·03045
·04	4	14 54·0	0·78230	1·04076	0·29276	0·04081
·05	5	16 19·7	0·80227	1·05111	0·33016	0·05127
·06	6	17 32·5	0·82270	1·06165	0·36483	0·06183
·07	7	18 35·0	0·84376	1·07224	0·39753	0·07250
·08	8	19 29·7	0·86531	1·08288	0·42874	0·08328
·09	9	20 17·8	0·88712	1·09365	0·45880	0·09416
·10	10	21 0·4	0·90944	1·10451	0·48795	0·10515
·11	11	21 38·4	0·93237	1·11548	0·51637	0·11625
·12	12	22 12·6	0·95578	1·12655	0·54421	0·12746
·13	13	22 43·0	0·97967	1·13771	0·57158	0·13878
·14	14	23 10·7	1·00408	1·14897	0·59858	0·15021
·15	15	23 35·6	1·02891	1·16027	0·62528	0·16176
·16	16	23 58·0	1·05437	1·17174	0·65174	0·17342
·17	17	24 18·3	1·08029	1·18329	0·67802	0·18520
·18	18	24 37·2	1·10649	1·19495	0·70416	0·19709
·19	19	24 54·0	1·13316	1·20670	0·73020	0·20910
·20	20	25 9·3	1·16038	1·21862	0·75617	0·22123
·21	21	25 23·5	1·18806	1·23058	0·78210	0·23348
·22	22	25 36·3	1·21607	1·24265	0·80802	0·24585
·23	23	25 48·2	1·24477	1·25490	0·83396	0·25833
·24	24	25 58·7	1·27397	1·26721	0·85994	0·27094
·25	25	26 8·7	1·30341	1·27962	0·88598	0·28367
·26	26	26 17·7	1·33343	1·29217	0·91209	0·29653
·27	27	26 26·0	1·36392	1·30482	0·93829	0·30951
·28	28	26 33·7	1·39483	1·31762	0·96459	0·32262
·29	29	26 40·7	1·42617	1·33052	0·99101	0·33586
·30	30	26 47·0	1·45801	1·34360	1·01756	0·34923
	$\lambda=$	28 1·3				

The curve thus calculated will be a type for all the forms which can arise when $e=2$, as they will only differ as regards the scale of measurement *.

When $B'=0$, the calculated results are as follows:—

* The Astronomer Royal has tabulated the coordinates, for the same value of e, on the hypothesis of the resistances varying simply as the velocity, which will be found to give to the curve too low a position, as might have been inferred by comparing the limiting angles exhibited in the foregoing Tables.

Table for $e=2$, $B'=0$, $\rho_0=1$.

q.	T−a.	x.	y.	q.	T−a.	x.	y.
0	1·00000	0·00000	0·00000	16	1·17351	0·65203	0·17351
1	1·01005	0·14265	0·01005	17	1·18530	0·67835	0·18530
2	1·02020	0·20349	0·02020	18	1·19721	0·70453	0·19721
3	1·03045	0·25138	0·03045	19	1·20924	0·73062	0·20924
4	1·04081	0·29279	0·04081	20	1·22140	0·75665	0·22140
5	1·05127	0·33020	0·05127	21	1·23368	0·78265	0·23368
6	1·06184	0·36488	0·06184	22	1·24608	0·80864	0·24608
7	1·07251	0·39759	0·07251	23	1·25860	0·83465	0·25860
8	1·08329	0·42881	0·08329	24	1·27125	0·86071	0·27125
9	1·09418	0·45888	0·09418	25	1·28402	0·88683	0·28402
10	1·10518	0·48805	0·10518	26	1·29692	0·91303	0·29692
11	1·11628	0·51650	0·11628	27	1·30996	0·93933	0·30996
12	1·12750	0·54437	0·12750	28	1·32313	0·96574	0·32313
13	1·13883	0·57177	0·13883	29	1·33643	0·99227	0·33643
14	1·15027	0·59880	0·15027	30	1·34986	1·01894	0·34986
15	1·16183	0·62553	0·16183				

The extreme coordinates contained in the first Table are $x=1·01756$, $y=0·34923$. From the second Table, when $B'=0$, for the same value of x the interpolated value of y is $0·34917$, being only $0·00006$ in defect, which would not be appreciable in any drawing of the curve; and for higher values of e the approximation will be still nearer.

Similar Tables might be constructed for other values of e.

From equations (10), (14), and (15) we have

$$\frac{y}{\mathrm{T}-a}=1-\left(\frac{1+\alpha}{1-\alpha}\cdot\frac{\cos\omega-\alpha}{\alpha\cos\omega+1}\right)^{\frac{1-\alpha^2}{1+\alpha^2}}=1-\left\{\frac{\tan(\theta-\mu)}{c_2}\right\}^{c_1}. \quad (16)$$

Or, putting $\dfrac{\mathrm{T}-a}{y}=\gamma$,

$$\tan(\theta-\mu)=c_2\left(\frac{\gamma-1}{\gamma}\right)^{\frac{1}{c_1}}. \quad . \quad . \quad . \quad (17)$$

When γ is taken equal to 1, 2, 3, &c., the values of ω calculated from this formula are shown in the following Table:—

Table of Inclinations (ω).

$\frac{T-a}{y}$	e=1.	e=2.	e=3.	e=4.	e=5.	e=6.	e=7.
1	51 49.6	28 1.3	18 55.2	14 15.0	11 25.0	9 31.0	8 11.0
2	46 24.5	27 58.3	,, ,,	,, ,,	,, ,,	,, ,,	,, ,,
3	40 42.4	27 30.0	18 54.8	,, ,,	,, ,,	,, ,,	,, ,,
4	36 30.7	26 39.2	18 52.2	,, ,,	,, ,,	,, ,,	,, ,,
5	33 20.8	25 40.3	18 45.0	14 14.7	,, ,,	,, ,,	,, ,,
6	30 52.0	24 41.3	18 34.2	14 13.7	,, ,,	,, ,,	,, ,,
7	28 51.7	23 45.2	18 20.0	14 11.8	,, ,,	,, ,,	,, ,,
8	27 12.0	22 53.4	18 3.5	14 9.0	11 24.7	,, ,,	,, ,,
9	25 47.5	22 5.6	17 46.0	14 5.0	11 24.0	,, ,,	,, ,,
10	24 34.8	21 21.6	17 28.0	14 0.0	11 23.0	,, ,,	,, ,,
11	23 31.4	20 41.6	17 9.8	13 54.7	11 22.0	9 30.5	,, ,,
12	22 35.6	20 4.5	16 51.8	13 48.3	11 20.7	9 30.0	,, ,,

Hence as a is a very small quantity and the tension cannot in practice exceed the tensile strength of the cable, we conclude that, if the depth of the sea be at all considerable, the inclination (ω) at the ship, must necessarily be very near to the limiting angle, especially if that angle be small.

If we consider the nature of the formula (11), it is evident that the quantity under the fractional exponent $\dfrac{1-\alpha^2}{1+\alpha^2}$ must necessarily be algebraically positive. Therefore $\cos\omega - \alpha$ and $\cos\omega_1 - \alpha$ must have like signs; that is, in any possible form of the cable, the inclination ω must either be always less or always greater than the limiting angle λ. Let us briefly examine these two cases separately.

1. When ω is always less than the limiting angle, the values of $\cos\omega - \alpha$ will be positive; and therefore the values of $T - a$ will be always positive. Also, since the values of $\cos\omega - e^2\sin^2\omega$ are positive, it follows from (3) that the radius of curvature is always positive. Consequently the curve is everywhere concave upwards and convex downwards.

The equation (12) indicates that the radius of curvature increases rapidly when ω approaches to the value of λ, and that the upper portion of the cable rapidly approximates in form to that of a straight lime. Also the equation (14) shows that for a given value of e, or at a given speed of the vessel, the contour of the curve will be given in species, the scale of measurement, or unit, being the length of the radius of curvature at the lowest point; and this again will depend upon the amount of "stray length" of cable payed out as compared with the depth of the sea.

Now
$$\frac{d(s-x)}{dy} = \frac{ds - dx}{dy} = \frac{1 - \cos\omega}{\sin\omega} = \tan\tfrac{1}{2}\omega;$$

$$\therefore \text{ stray length } s - x = \int dy \tan\tfrac{1}{2}\omega. \quad \cdot \quad \cdot \quad \cdot \quad (18)$$

As ω is always less than λ, the superior limit of this integral is obviously $y\tan\tfrac{1}{2}\lambda$, or $c_5 y$. For this extreme limit we must have $\omega = \lambda$; and if this equality exist at any point where the tension is finite, it must, by (11), subsist at all other points; and the curve will therefore merge into a straight line. And by (14) we have then $\rho_0 = 0$, and by (3) $T_0 = a$, which is the least possible value of the tension at the lowest point. But as the curve should be continuous with the line of the deposited cable, in order that this tension may be adequately sustained, we ought not to pay out the stray length to the extreme limit, but only until ρ_0 becomes small; for the prescribed movement of any portion of the cable, separately considered, must obviously require

that the due amounts of tension shall be continually maintained at its extremities.

The cable being traced upwards from the lowest point soon assimilates to a straight line, and when the stray length is gradually in-

creased the modifications of the curve will be as in the annexed diagram, where $B'D'$ is the direction of the ship's motion, and AB, ac, $a'd$ successive representations of the suspended cable. The curves being similar, AB will be a prototype of the rapidly decreasing portions ab, $a'b'$, and the diminished scale of measurement bringing in an increased proportion of the upper part of the curve, it will evidently approximate to the right line mn as a limit, where ρ_0 becomes evanescent.

In each position of the curve we have by (3) $T_0 = a + \rho_0$, and at the limit $T_0 = a$, which we have observed is the minimum tension at the lowest point. The Astronomer Royal, from confounding $T - a$ with the tension T, has presumed the non-existence of tension at this point; and subsequently Mr. Homersham Cox, in a paper read before the British Association, after advancing the same assumption as an original principle, has adduced a somewhat ingenious and elaborate proof of the dynamical necessity of the descending cable taking the form of a straight line; but, it will appear, both the premises and conclusion are equally inadmissible.

2. When ω is greater than the limiting angle λ, $\cos \omega - \alpha$ will be negative, and by (11), if $T - a$ be positive at one point, it will be positive at all points. Also the value of $\cos \omega - e^2 \sin^2 \omega$ is negative, and by (3) ρ is negative, and hence the curve is always convex upwards. But as the direction of the curve at the lowest point (A') is not continuous with the line of cable previously deposited, there cannot be any stay or reaction to support the requisite tension at that point, and the consequence will be a distorted movement in this locality accompanied by an irregular displacement and a useless and extravagant waste of a portion of the cable. Such will be the inevitable result of a profuse paying out of the cable; and other injurious consequences may ensue from the friction occasioned by the cable being dragged to a certain distance along the bed of the ocean before the tension due to a steady movement can be reinstated.

If, therefore, a Table be made out showing the weight, in water, of any number of fathoms of cable, it will be an important and valuable rule for practical guidance, always to moderate the paying out so as to keep the tension a certain proportion, say one-third or one-fourth, in excess of the weight corresponding to the
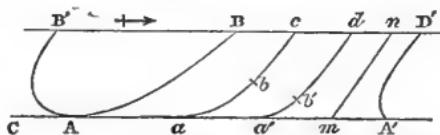
ascertained depth of the sea. By so doing, the tension will be kept within reasonable limits, the curve will always be slightly concave upwards, and there will be no waste. We also conclude that the mechanical structure and dimensions of the cable ought to be such as to enable it to bear a tensile strain of not less than three times the maximum depth.

In calculations of distance, it will naturally occur that an additional length of cable will be needed to accommodate the irregularities of the bed of the ocean; and it may not be unreasonable to entertain a conjecture as to whether or not any extensive fissures or chasms may exist at great depths, which can only be ascertained by repeated soundings.

Note.—The mechanical or dynamical conditions which determine the subsistence of both branches of the curve of steady movement, may be comprehended by conceiving two vessels, B′ B, to be sailing with equal velocities in the direction B′ B, and that the suspended cable curve B′ A B is payed out from B and at the same time wound up to B′ with the same velocity, so as to keep the lowest point A just on the verge of contact with the ground. The curve will be nearly the same in form whichever of the two be the paying out vessel; and when B and B′ are supposed to coincide, the two branches of the curve will become duplicates of the limiting line *m n*.

Alwyne Lodge, Canonbury,
April 5, 1860.

L. *On certain Laws of Chromatic Dispersion.*
By Mungo Ponton, *F.R.S.E.*
[Concluded from p. 272.]

THE difference between the view of M. Cauchy, then, and that arising out of the foregoing investigation, may be shortly stated thus:—According to the former, the refractive index of the wave corresponding to each of the fixed lines is a compound quantity consisting of *two* terms, one constant for the medium and temperature, the other variable and inversely proportional to the squares of the normal wave-length. According to the latter view, the refractive index is still a compound quantity, but consisting of *three* instead of only *two* terms. One is constant for the medium and temperature. The second is variable—*not*, however, in inverse proportion to the squares of the normal wave-lengths. It corresponds to a further shortening of the wave-length beyond that corresponding to the constant portion of the index; and these further shortenings are in strict inverse proportion to the primary normal wave-lengths, or to the initial forces by which these are generated. The third portion of the index is also variable, and is that corresponding to those varia-

tions in the wave-lengths which are rendered manifest by the displacement of the fixed lines from their normal relative positions in the unrefracted spectrum, and which are attendant on the irrationality. The first, or constant, portion of the index indicates the state of compression of the æther in the pores of the medium. The variable fragment consists of two portions,—one depending for its magnitude on the specific action of the ponderable molecules of the medium on the æther within its pores, the other apparently depending on a specific action of the *vibrations* of the ponderable molecules of the medium on certain definite waves passing through the æther in its pores, in virtue of which some of these waves are slightly lengthened, and others slightly shortened, but always in a certain symmetrical manner.

To place the matter in a *popular* point of view :—Let the æther be regarded as a universally diffused elastic fluid, and each portion of it as comprising numerous centres of elastic or repulsive force. Since force cannot exist as a simple abstraction (for we cannot conceive of force as being exerted by nothing, or by mere space), it is needful to suppose these centres to be each occupied by a *something* which exerts the force, and which may be called 'an æthereal particle.' These particles must be separated from each other by certain minute but variable distances, otherwise there could be no movement of the particles. Each particle must be conceived to occupy a certain normal position in absolute space, from which it never departs except under the influence of some applied external force, and to which it always returns when that applied force ceases to act upon it. In the free æther, each particle is retained in its normal position with a certain persistence, in virtue of the forces exerted upon it by all the other æthereal particles by which it is surrounded. The force required to induce any given amount of motion in an æthereal particle must be proportional to the degree of this persistence.

Assume, for the sake of illustration, that in the wave corresponding to the fixed line B in the free æther there are embraced in the direction of its length 1,000,000 of such particles, that is, the moving force has time to progress onwards, and more or less to affect 1,000,000 particles in a right line in the direction of propagation, during the period occupied by a single particle in performing its individual motion in this particular wave. On this assumption, the wave corresponding to the fixed line H must similarly embrace, in the direction of its length, 570,655 such particles. Suppose now a portion of the æther to be, from any cause, so compressed as to halve the intervals between its particles. When the waves B and H enter this compressed portion, B ought still to embrace 1,000,000, and H 570,655 particles in their respective lengths; but the intervals

between the particles being halved, each of those waves would now be reduced to one-half of its original length. When the æther is compressed in the pores of a refracting medium, however, this rule does not hold. The B wave now appears to embrace in its length less than 1,000,000, and the H wave less than 570,655 particles. If the B wave thus seem to lose, say 50,000 of its particles in the direction of its length, the H wave will seem to lose exactly the same number; so that this loss tells very differently on the lengths of these two waves. The H wave becomes *proportionally* more shortened by this loss than the B wave; so that the motive force progresses more slowly in the H wave, in proportion to its rate of progress in the B wave, than it did before entering the medium. Hence to keep up with its more rapid neighbour, and present a straight front to the observer, the H wave takes *a shorter cut* in passing through the medium. It pursues a different path; the two waves become more or less separated, and this constitutes *dispersion*.

Now this want of power in the motive force to extend to its full number of particles during the period of the individual excursion of each particle, must be due to an increase in the persistence of the particles in their normal positions. This persistence will of course be increased by the greater proximity of the particles; but the increase of persistence thence arising exhibits itself in the general shortening of the wave-length, in proportion to the compression of the æther. Were the additional loss of length which manifests itself in the dispersion of the waves due to this cause alone, then would the dispersive power of every medium be proportional to the density of the æther in its pores, or to its general refractive power. But this is far from being the case. The oil of cassia, for example, which exerts a smaller compressing force on the æther than does crown-glass, has nevertheless a much higher dispersive power; so that the latter must be due to some other force than that which causes the refraction.

Suppose now a medium in which the value of $a\epsilon$ is 0·05, and that of ϵ 1·5, so making the density of the æther one-half more than it is in the planetary spaces, the intervals between the æthereal particles being thus reduced in the proportion of 0·66 to 1. Were there no other cause operating than this greater proximity of the particles, and if the wave-length continued to embrace its primary number of particles, each wave-length ought to be diminished in this same proportion, at least very nearly so; for some little allowance must be made for the size of the particles as compared with that of the intervals. But it is found that, leaving out of view that peculiar property of the medium which exhibits itself in the irrationality, and confining attention to its *dispersive* power alone, the number of particles embraced

in the B wave on entering such a medium will appear to be only 950,000 instead of 1,000,000, while in the H wave the number will appear to be only 520,655 instead of 570,655; so that each of these waves has had its length curtailed by the space which 50,000 æthereal particles would occupy in this medium, in addition to the curtailment, in the proportion of 0·66 to 1, due to the increased density of the æther. Now this must be owing to the circumstance that the molecules of the medium exert on the æthereal particles in its pores an influence such as to increase their tendency to remain in their normal positions, to a certain extent beyond the measure in which that tendency is increased, by the mere approximation of the æthereal particles themselves. By the operation of this force, the B wave has its length diminished in the proportion of 0·95 to 1, but the H wave in the proportion of nearly 0·91238 to 1; so that in the case of the B wave the *loss* is in the proportion of 0·05 to 1, but in the H wave it is 0·08762 to 1, these losses standing to each other in inverse proportion to the initial moving forces; and the like may be proved with respect to all the other wave-lengths. Thus the loss of length arising from that peculiar action of the ponderable molecules of the medium on the æther in its pores, in virtue of which it increases the persistence of the æthereal particles in their normal positions, is in exact inverse proportion to the amount of the primary force by which the undulation is excited; and this is precisely what ought to be expected. The *absolute* loss of length sustained by each wave from this cause is the same, but its *relative* loss is in inverse proportion to its primary length. Thus *dispersion* arises from the operation of a force in virtue of which the ponderable molecules of the medium communicate to the æthereal particles, which are for the instant associated with them, a certain amount of persistence in their normal positions beyond what is due to the increased proximity of the particles themselves. It is *constant* for the medium and temperature; but it is *specific*, and independent of the size of the pores of the medium in which the æther is momentarily more or less compressed; so that a high *dispersive* power may consist with a low *refractive* power, or *vice versâ*; although *in general* the greater the *compressive* power of the medium, the greater the amount of this peculiar force.

This, then, accounts for the larger proportion of the variable quantity which remains after deducting from the indices corresponding to the fixed lines the constant which represents the density of the æther in the pores of the medium. That variable portion corresponds to a loss of wave-length inversely proportional to the primary wave-length, and may be found by multiplying the index by this loss, as already pointed out in the case

of bisulphuret of carbon. But the indices thus obtained are
subject to a further correction, by the addition or subtraction of
that portion which is due to the extrusion of the fixed lines
attending the irrationality of the spectrum.

The remaining portion of the variable part of the index cor-
responds to this extrusion of the fixed lines from their normal
positions, in consequence of which their mutual distances are
altered from what they are in the unrefracted spectrum. This
is quite a distinct phænomenon from dispersion, which is simply
the greater or smaller expansion of the coloured spaces, and the
consequent greater or smaller length of the spectrum which the
medium presents. Besides the shortening of the wave, attribu-
table to the increased density of the æther within the medium,
and the further shortening which it undergoes by losing a defi-
nite number of the particles embraced in its length, it may be
still further modified by an alteration in the period of vibration
of each of its component æthereal particles,—an alteration which
will cause a corresponding small variation in the length of the
wave, and in the position of the fixed line corresponding to it.
These alterations, it has been shown, always take place accord-
ing to certain determinate laws; and the total amount of the
positive and negative extrusions is so related to the dispersive
power, that, by means of the ratio between these two, there may
be always found for the normal wave-lengths an exponent at which
the extrusions are reduced to zero, this exponent being the above
ratio multiplied by 10·8 and added to unity. By applying this
exponent to the normals, the indices of refraction become reduced
to *two* terms instead of *three*, as already pointed out.

In explanation of the irrationality two views may be suggested.
The displacement of the fixed lines is obviously due to the cir-
cumstance that the waves corresponding to the central lines D,
E, and F are less refracted; while those corresponding to the
extreme lines B, C, G, and H are more refracted than they
would otherwise be. There is thus a transfer of motive energy
from the extremes to the central region of the spectrum. Now
this distribution corresponds to that of the brightness; for all
spectra are brighter towards the centre than towards the extremes.
The brightness being proportional to the squares of the ampli-
tudes of the individual vibrations embraced in the wave-length,
it follows that, at the recipient surface, the amplitudes of those
vibrations are greater towards the centre than towards the ex-
tremes of the spectrum. This correspondence raises a presump-
tion of there being some connexion between these two phæno-
mena. The medium may be supposed to produce a certain effect
on the amplitudes of the vibrations, decreasing them by a certain
definite amount, and increasing the rapidity of the vibration and

the refrangibility of the wave by a like definite amount, as a consequence of the curtailment of the amplitude. This curtailment being absolutely the same for all the waves, will produce the greatest rateable effect on those undulations whose vibrations have the least amplitude, namely those at the extremes of the spectrum. Hence these will have their refraction increased from the operation of this cause in a higher proportion than will the central waves, and so produce a displacement of the fixed lines from their normal positions in the manner observed.

Or again, the slight alterations in the rapidity of the individual vibrations of the æthereal particles comprised within the wave-length, and which manifest themselves by the displacement of the fixed lines, may be due to a sympathetic action between the vibrations of the ponderable atoms of the medium and those of the æthereal particles, somewhat resembling the sympathy of vibrating pendulums; and in virtue of this sympathy some of the æthereal vibrations may be slightly increased, and others slightly diminished in their rate of rapidity beyond what they would otherwise be. These views, however, are merely thrown out as hints for consideration.

While one of the objects with which the foregoing investigation is submitted to the British Association is to call the attention of its members to the curious laws governing the displacement of the fixed lines, and their use in detecting errors of observation, and to bring under their notice the advantages of the exponential law as furnishing a method of calculating accurate indices of refraction from tolerably correct observations, the chief purpose is to awaken those who take a lead in the proceedings of the Association to the necessity which exists for a careful repetition of those observations that have been found most deficient in accuracy, and for an extension of the observations generally. It would be particularly interesting to accumulate observations at different temperatures, so as further to illustrate the law of the variation of the index of elasticity, viewed in relation to the contractions and expansions of the medium under the influence of changes of temperature, and also in relation to the capacity of the medium for heat. It is not improbable that, out of an experimental investigation carefully conducted in this direction, some highly interesting and important results might arise.

It is only under the direction and auspices of such a body as the British Association that there is any likelihood of an experimental investigation of such a nature being undertaken; and it is hoped that, when its importance to the science of physical optics shall have been duly weighed by them, they will not fail to place the matter in the hands of competent and careful observers, so that an enlarged store of trustworthy experimental data may be obtained.

TABLE I.—Elements of Calculation.
The media arranged according to the value of $\log e$.

Media	$\log \epsilon.$	$a.$	$n.$	$\log \epsilon_{n}.$	$a_{n}.$
P. Nitrate of bismuth	0·1185050	0·009597	1·7	0·1214201	0·004622
F. Water, No. 1, T. 18°·75...	0·1185492	0·009687	1·6	0·1212442	0·005087 ⎫ *
F. Water, No. 2, T. 18°·75...	0·1185793	0·009645	...	0·1212442	0·005087 ⎭
P. Water, T. 15°·8	0·1187071	0·009836	2·0	0·1223208	0·003694
P. Muriate of zinc	0·1190925	0·012365			
P. Subacetate of lead........	0·1199078	0·009548	1·8	0·1230654	0·004192
P. Nitrate of mercury	0·1210345	0·010891	...	0·1246282	0·004806
P. Sulphate of soda	0·1210587	0·009894	1·6	0·1238184	0·005206
P. Muriate of baryta	0·1213789	0·009615	2·6	0·1257358	0·002316
P. Superacetate of lead	0·1221380	0·010067	1·9	0·1256868	0·004083
P. Nitrate of potash	0·1225076	0·011004	2·4	0·1273767	0·002970
P. Sulphate of magnesia ...	0·1225266	0·009738	1·4	0·1246065	0·006206
P. Nitrate of lead	0·1227524	0·010397	1·8	0·1262004	0·004583
P. Muriate of ammonia	0·1240384	0·010655	1·8	0·1275798	0·004701
P. Alcohol, T. 17°·6	0·1288364	0·009368	1·7	0·1317568	0·004504
P. Pyroligneous acid	0·1315513	0·010312	2·0	0·1354593	0·003869
P. Nitric acid	0·1370585	0·014199	2·1	0·1427913	0·004907
P. Muriate of lime............	0·1389150	0·012103	1·9	0·1433528	0·004911
P. Muriatic acid, T. 18°·6 ...	0·1392094	0·013901	1·9	0·1422945	0·008862
F. Sol. of Potash, T. 21°·5 ...	0·1392960	0·011002	1·7	0·1428138	0·005287
P. Solution of soda	0·1396349	0·012410	1·6	0·1432346	0·006549
P. Solution of potash, T. 16°.	0·1398999	0·011513	1·4	0·1424568	0·007341
P. Sulphuric acid, T. 18°·6 ..	0·1502045	0·009301	1·0	0·1502375	0·009259
F. Oil of turpentine, T. 10°·6..	0·1585949	0·013758	2·1	0·1644359	0·004751
R. Calc-spar ex. ray	0·1661054	0·008120	...	0·1695691	0·002824
P. Oil of cassia, No. 3, T. 22°·5.	0·1671802	0·049533	3·4	0·1966605	0·006945
P. Oil of sassafras, T. 20°·9 ..	0·1680534	0·023053	2·7	0·18c0982	0·005144
P. Oil of anise, T. 20°·9......	0·1683381	0·030529	2·8	0·1847136	0·006356
P. Oil of cassia, T. 14°	0·1688690	0·048800	3·5	0·1982254	0·006483
P. Oil of anise, T. 15°·1......	0·1695285	0·030191	2·8	0·1857498	0·006297
P. Oil of cassia, T. 10°	0·1695982	0·048723	3·2	0·1978781	0·007803
P. Oil of anise, T. 13°·25 ...	0·1698189	0·029579	2·8	0·1857218	0·006165
P. Creosote	0·1699152	0·022836	2·5	0·1813500	0·005855
F. Crown-glass No. 13	0·1755898	0·011225	1·9	0·1800728	0·004550
F. Crown-glass No. 9	0·1759127	0·011384	1·9	0·1804622	0·004615
P. Rock-salt	0·1771542	0·015460	2·2	0·1842930	0·004942
R. Arragonite, 1st axis	0·1785369	0·008122	1·9	0·1817994	0·003291
R. Quartz O. ray.............	0·1814768	0·009365	1·7	0·1847752	0·004501
P. Bisulphide of carbon......	0·1819878	0·038772	2·5	0·2021429	0·009949
P. Crown-glass M.............	0·1828213	0·013003	2·0	0·1883987	0·004857
P. Quartz ex. ray	0·1838501	0·009541	1·7	0·1872281	0·004586
F. Flint-glass No. 3	0·1915881	0·018721	2·2	0·2005423	0·005986
F. Flint-glass No. 30	0·1961852	0·020178	2·2	0·2059514	0·006450
F. Flint-glass No. 23 (2) ...	0·1967997	0·020402	2·2	0·2066932	0·006518 ⎫ *
F. Flint-glass No. 23 (1) ...	0·1968160	0·020382	2·2	0·2066932	0·006518 ⎭
F. Flint-glass No. 13	0·1970545	0·020502	2·1	0·2066058	0·007080
R. Topaz, 2nd axis............	0·2004488	0·008465	1·7	0·2035624	0·004069
R. Topaz, 3rd axis	0·2010442	0·008421	1·7	0·2041424	0·004051
R. Topaz, 1st axis	0·2029670	0·008466	1·8	0·2063425	0·003735
R. Calc-spar O. ray	0·2081261	0·014107	1·9	0·2142084	0·005719
R. Arragonite, 3rd axis	0·2148137	0·013062	1·9	0·2205295	0·005297
R. Arragonite, 2nd axis......	0·2156888	0·013330	2·0	0·2218703	0·004971

F = Fraunhofer.　P = Powell.　R = Rudberg.

* Mean of two observations.
Note.—The elements $\log \epsilon_n$, a_n, and n are the three from which the indices in Table VI. are calculated. The elements $\log \epsilon$ and a are those from which the wave-lengths in Table III. are calculated.

TABLE II.—Internal Wave-lengths calculated from the observed Indices (corrected by the laws of extrusion) per formula $\frac{U}{\mu U} = u$.

Media in same order as in Table I.

Media	B.	C.	D.	E.	F.	G.	H.	Sum.
Space	1·000000	0·953893	0·856059	0·764567	0·704210	0·623398	0·570655	5·472782
	b.	*c.*	*d.*	*e.*	*f.*	*g.*	*h.*	
trate of bismuth ...	0·751542	0·716406	0·642103	0·572495	0·526556	0·464874	0·424688	4·098664
ater(1),T.18°·75...	0·751350	0·716291	0·641927	0·572345	0·526387	0·464774	0·424538	4·097612
ater(2),T.18°·75...	0·751328	0·716293	0·641927	0·572343	0·526399	0·464785	0·424543	4·097618
ater, T. 15°·8	0·750920	0·715815	0·641571	0·572108	0·526087	0·464217	0·424337	4·095055
uriate of zinc	0·749008	0·711755	0·637850	0·568703	0·522954	0·461742	0·421645	4·073657
bacetate of lead...	0·749064	0·714153	0·640121	0·570654	0·524880	0·463389	0·423315	4·085576
trate of mercury....	0·745825	0·710853	0·636996	0·567945	0·522140	0·460820	0·420837	4·065416
lphate of soda......	0·746734	0·711941	0·637943	0·568789	0·523112	0·461814	0·421830	4·072163
uriate of baryta....	0·746378	0·711546	0·637848	0·568960	0·522956	0·461635	0·421741	4·071064
peracetate of lead..	0·744657	0·709901	0·636238	0·567187	0·521715	0·460480	0·420495	4·060673
trate of potash	0·743108	0·708266	0·634751	0·565924	0·520346	0·458853	0·419351	4·050599
lphate of magnesia.	0·744380	0·709637	0·635907	0·566935	0·521483	0·460412	0·420527	4·059281
trate of lead	0·743216	0·708630	0·634973	0·566093	0·520548	0·459462	0·419600	4·052522
uriate of ammonia.	0·740796	0·706169	0·632758	0·564172	0·518755	0·457808	0·417900	4·038520
cohol, T. 17°·6......	0·733783	0·699693	0·626965	0·559095	0·514172	0·453941	0·414689	4·002338
roligneous acid......	0·728385	0·693994	0·622136	0·554637	0·510039	0·450172	0·411016	3·970379
tric acid	0·714899	0·681450	0·610336	0·543713	0·499724	0·440408	0·402330	3·892230
uriate of lime	0·713980	0·680574	0·609728	0·543240	0·499475	0·440564	0·402152	3·889875
uriatic acid	0·711737	0·678449	0·607368	0·541091	0·497310	0·438485	0·400153	3·874598
l.potash,T.21°·5...	0·714475	0·681102	0·610248	0·543934	0·500120	0·441319	0·402909	3·894098
lution of soda	0·712458	0·679464	0·608197	0·541899	0·498254	0·439598	0·401273	3·881143
l. potash, T. 16°...	0·713063	0·679605	0·608817	0·542590	0·498839	0·440191	0·401898	3·885003
lphuric acid.........	0·698275	0·665708	0·596515	0·531691	0·489035	0·431716	0·394561	3·807501
. of turpentine	0·680043	0·648233	0·580602	0·517176	0·475260	0·418894	0·381996	3·702204
lc-spar ex. ray......	0·673900	0·642569	0·575966	0·513583	0·472402	0·417128	0·380995	3·676543
l of cassia, T.22°·5..	0·629128	0·598803	0·534169	0·472714	0·431660	0·374976	0·335976	3·377426
l of sassafras	0·655438	0·624480	0·558749	0·496889	0·455858	0·400255	0·363636	3·555305
l of anise, T. 20°·9..	0·647207	0·616489	0·551087	0·489388	0·448342	0·392543	0·355482	3·500538
l of cassia, T. 14°...	0·627156	0·596967	0·532607	0·471748	0·430499	0·373941	0·335186	3·368104
l of anise, T. 15°·1...	0·645744	0·615098	0·549742	0·488261	0·447317	0·391778	0·354796	3·492736
l of cassia, T. 10°...	0·626449	0·595922	0·531582	0·470532	0·429685	0·373337	0·334911	3·362418
l of anise, T.13°·25..	0·645912	0·615257	0·549999	0·488541	0·447660	0·392049	0·355194	3·494544
eosote	0·652742	0·622037	0·556497	0·494800	0·453800	0·398617	0·362343	3·540926
own-glass No. 13...	0·656034	0·625381	0·560254	0·499267	0·458967	0·404828	0·369431	3·574162
own-glass No. 9	0·655380	0·624746	0·559667	0·498737	0·458455	0·404369	0·368982	3·570336
ck-salt...............	0·649222	0·618808	0·554160	0·493330	0·453126	0·399052	0·363684	3·531382
ragonite, 1st axis....	0·654665	0·624194	0·559479	0·498868	0·458829	0·405118	0·370026	3·571179
uartz O. ray	0·648917	0·618688	0·554371	0·494194	0·454446	0·401105	0·366227	3·538002
sulphide of carbon..	0·617971	0·588133	0·524932	0·465125	0·425376	0·371092	0·335285	3·327914
own-glass M	0·643180	0·613070	0·549081	0·489117	0·449475	0·396177	0·361295	3·501395
uartz ex. ray........	0·645203	0·615098	0·551123	0·491272	0·451735	0·398694	0·363607	3·517132
int-glass No. 3	0·624051	0·594771	0·532211	0·473553	0·434687	0·382272	0·347881	3·389580
int-glass No. 30.....	0·615927	0·586839	0·525001	0·466955	0·428491	0·376583	0·342515	3·342311
int-glass No. 23(2)..	0·614793	0·585767	0·524010	0·466044	0·427629	0·375801	0·341775	3·335819
int-glass No. 23(1)..	0·614781	0·585762	0·524011	0·466062	0·427655	0·375802	0·341774	3·335827
int-glass No. 13	0·614345	0·585325	0·523572	0·465621	0·427245	0·375476	0·341492	3·333076
paz, 2nd axis........	0·621736	0·592738	0·531186	0·473564	0·435504	0·384457	0·351086	3·390271
paz, 3rd axis........	0·620983	0·591966	0·530494	0·472916	0·434939	0·383960	0·350654	3·385854
paz, 1st axis........	0·618085	0·589261	0·528073	0·470763	0·432961	0·382171	0·349003	3·370317
lc-spar O. ray	0·604925	0·576545	0·516164	0·459589	0·422189	0·371911	0·339009	3·290332
ragonite, 3rd axis...	0·596552	0·568538	0·509073	0·433402	0·416569	0·367050	0·334675	3·245859
ragonite, 2nd axis...	0·595026	0·567120	0·507776	0·452193	0·415439	0·366016	0·333697	3·237267

TABLE III.—Internal Wave-lengths, freed from the extrusions,

calculated by formula $\dfrac{U}{\epsilon} - a = u.$

Media in same order as in Table I.

Media	B.	C.	D.	E.	F.	G.	H.	Sum.
Space	1·000000	0·953893	0·856059	0·764567	0·704210	0·623398	0·570655	5·472782
	b_2.	c_2.	d_2.	e_2.	f_2.	g_2.	h_2.	
Nitrate of bismuth	0·751596	0·716500	0·642029	0·572386	0·526443	0·464929	0·424781	4·098664
Water, No. 1, T.18°·75	0·751429	0·716336	0·641873	0·572237	0·526299	0·464791	0·424647	4·097612
Water, No. 2, T.18°·75	0·751418	0·716328	0·641870	0·572239	0·526304	0·464800	0·424659	4·097618
Water, T. 15°·8	0·751003	0·715924	0·641487	0·571876	0·525955	0·464470	0·424340	4·095055
Muriate of zinc	0·747799	0·712751	0·638380	0·568831	0·522950	0·461520	0·421426	4·073657
Subacetate of lead	0·749191	0·714208	0·639977	0·570558	0·524764	0·463448	0·423430	4·085576
Nitrate of mercury	0·745882	0·711099	0·636951	0·567712	0·522036	0·460880	0·420965	4·065416
Sulphate of soda	0·746836	0·711947	0·637912	0·568677	0·523003	0·461850	0·421938	4·072163
Muriate of baryta	0·746558	0·711693	0·637713	0·568530	0·522890	0·461782	0·421898	4·071064
Superacetate of lead	0·744785	0·709982	0·636131	0·567068	0·521508	0·460506	0·420693	4·060673
Nitrate of potash	0·743206	0·708432	0·634644	0·565640	0·520119	0·459169	0·419389	4·050599
Sulphate of magnesia	0·744439	0·709667	0·635882	0·566881	0·521361	0·460414	0·420637	4·059281
Nitrate of lead	0·743388	0·708634	0·634887	0·565922	0·520426	0·459511	0·419754	4·052523
Muriate of ammonia	0·740901	0·706251	0·632721	0·563960	0·518599	0·457864	0·418224	4·038520
Alcohol	0·733931	0·699660	0·626940	0·558934	0·514071	0·454003	0·414799	4·002338
Pyroligneous acid	0·728355	0·694298	0·622030	0·554448	0·509865	0·450171	0·411212	3·970379
Nitric acid	0·715160	0·681532	0·610175	0·543445	0·499423	0·440482	0·402013	3·892230
Muriate of lime	0·714145	0·680660	0·609608	0·543162	0·499328	0·440638	0·402334	3·889875
Muriatic acid	0·711855	0·678393	0·607389	0·540988	0·497184	0·438534	0·400255	3·874598
Sol. of potash, T. 21°·5	0·714609	0·681154	0·610164	0·543776	0·499981	0·441343	0·403071	3·894098
Solution of soda	0·712635	0·679206	0·608271	0·541935	0·498174	0·439582	0·401340	3·881143
Sol. of potash, T. 16°	0·713090	0·679681	0·608790	0·542494	0·498760	0·440203	0·401985	3·885003
Sulphuric acid	0·698311	0·665686	0·596457	0·531716	0·489007	0·431823	0·394501	3·807501
Oil of turpentine	0·680315	0·648814	0·580409	0·516907	0·475015	0·418926	0·382318	3·702204
Calc-spar ex. ray	0·674053	0·642600	0·575860	0·513447	0·472273	0·417145	0·381165	3·676543
Oil of cassia, T. 22°·5	0·630954	0·599579	0·533004	0·470745	0·429673	0·374681	0·338790	3·377426
Oil of sassafras	0·656067	0·624755	0·558314	0·496180	0·455190	0·400309	0·364440	3·555305
Oil of anise, T. 20°·9	0·648146	0·616855	0·550457	0·488363	0·447401	0·392556	0·356750	3·500538
Oil of cassia, T. 14°	0·629046	0·597793	0·531476	0·469459	0·428546	0·373768	0·338016	3·368104
Oil of anise, T. 15°·1	0·646626	0·615421	0·549204	0·487281	0·446431	0·391735	0·356038	3·492736
Oil of cassia, T. 10°	0·627986	0·596785	0·530579	0·468666	0·427822	0·373136	0·337444	3·362418
Oil of anise, T. 13°·25	0·646786	0·615601	0·549429	0·487547	0·446724	0·392065	0·356392	3·494544
Creosote	0·653379	0·622201	0·556044	0·494176	0·453362	0·398715	0·363049	3·540926
Crown-glass No. 13	0·656212	0·625439	0·560142	0·499075	0·458791	0·404854	0·369651	3·574162
Crown-glass No. 9	0·655557	0·624807	0·559557	0·498537	0·458283	0·404386	0·369209	3·570336
Rock-salt	0·649577	0·618914	0·553851	0·493005	0·452866	0·399123	0·364046	3·513382
Arragonite, 1st axis	0·654801	0·524236	0·559379	0·498727	0·458715	0·305143	0·370178	3·571179
Quartz O. ray	0·649086	0·618727	0·554307	0·494064	0·454323	0·401112	0·366383	3·538002
Bisulphide of carbon	0·618904	0·588581	0·524238	0·464065	0·424370	0·371222	0·336534	3·327914
Crown-glass M	0·643412	0·613147	0·548927	0·488870	0·449251	0·396205	0·361583	3·501395
Quartz ex. ray	0·645321	0·615128	0·551060	0·491145	0·451620	0·398699	0·364159	3·517132
Flint-glass No. 3	0·624577	0·594916	0·531979	0·473123	0·434296	0·382309	0·348380	3·389580
Flint-glass No. 30	0·616346	0·586998	0·524724	0·466487	0·428069	0·376630	0·343057	3·342311
Flint-glass No. 23 (2)	0·615222	0·585916	0·523730	0·465575	0·427211	0·375845	0·342320	3·335819
Flint-glass No. 23 (1)	0·615218	0·585913	0·523729	0·465577	0·427214	0·375850	0·342326	3·335827
Flint-glass No. 13	0·614749	0·585460	0·523331	0·465190	0·426848	0·375512	0·342007	3·333076
Topaz, 2nd axis	0·621841	0·592779	0·531114	0·473446	0·435403	0·384466	0·351222	3·390271
Topaz, 3rd axis	0·621021	0·592069	0·530419	0·472830	0·434839	0·383972	0·350773	3·385854
Topaz, 1st axis	0·618189	0·589302	0·527993	0·470658	0·432835	0·382193	0·349141	3·370317
Calc-spar O. ray	0·605154	0·576602	0·516017	0·459360	0·421983	0·371939	0·339277	3·290332
Arragonite, 3rd axis	0·596736	0·568621	0·508961	0·453170	0·416364	0·367085	0·334922	3·245869
Arragonite, 2nd axis	0·595241	0·567182	0·507643	0·451963	0·415232	0·366052	0·333954	3·237267

TABLE V.*—Logarithms of the Wave-lengths of the fixed lines for each exponent from 1 to 3·5, that of B being =0. Also the Logarithms of S and Δ.

n.	C.	D.	E.	F.	G.	H.	S.	Δ.
1·0	1̄·9794999	1̄·9325036	1̄·8834154	1̄·8477024	1̄·7947653	1̄·7563732	0·7382081	0·3224004
1·1	1̄·9774499	1̄·9257540	1̄·8717569	1̄·8324726	1̄·7742418	1̄·7320115	0·7284077	0·3531264
1·2	1̄·9753999	1̄·9190043	1̄·8600985	1̄·8172429	1̄·7537184	1̄·7076478	0·7187648	0·3799546
1·3	1̄·9733499	1̄·9122547	1̄·8484400	1̄·8020131	1̄·7331949	1̄·6832852	0·7092784	0·4040377
1·4	1̄·9712999	1̄·9055050	1̄·8367816	1̄·7867834	1̄·7126714	1̄·6589225	0·6999472	0·4256320
1·5	1̄·9692498	1̄·8987554	1̄·8251231	1̄·7715536	1̄·6921479	1̄·6345598	0·6907696	0·4450947
1·6	1̄·9671998	1̄·8920058	1̄·8134646	1̄·7563238	1̄·6716245	1̄·6101971	0·6817445	0·4627112
1·7	1̄·9651498	1̄·8852561	1̄·8018061	1̄·7410940	1̄·6511010	1̄·5858344	0·6728703	0·4787166
1·8	1̄·9630998	1̄·8785064	1̄·7901477	1̄·7258643	1̄·6305775	1̄·5614717	0·6641455	0·4933033
1·9	1̄·9610498	1̄·8717568	1̄·7784893	1̄·7106346	1̄·6100524	1̄·5371091	0·6555684	0·5066368
2·0	1̄·9589998	1̄·8650072	1̄·7668308	1̄·6954048	1̄·5895306	1̄·5127464	0·6471379	0·5188511
2·1	1̄·9569498	1̄·8582576	1̄·7551723	1̄·6801750	1̄·5690071	1̄·4883837	0·6388519	0·5300649
2·2	1̄·9548998	1̄·8515079	1̄·7435139	1̄·6649453	1̄·5484837	1̄·4640210	0·6307087	0·5403794
2·3	1̄·9528498	1̄·8447583	1̄·7318554	1̄·6497155	1̄·5279602	1̄·4396584	0·6227067	0·5498820
2·4	1̄·9507998	1̄·8380086	1̄·7201970	1̄·6344858	1̄·5074367	1̄·4152957	0·6148442	0·5586479
2·5	1̄·9487497	1̄·8312590	1̄·7085385	1̄·6192560	1̄·4869132	1̄·3909330	0·6071192	0·5667425
2·6	1̄·9466997	1̄·8245094	1̄·6968800	1̄·6040262	1̄·4663898	1̄·3665703	0·5995298	0·5742271
2·7	1̄·9446497	1̄·8177597	1̄·6852215	1̄·5887964	1̄·4458663	1̄·3422076	0·5920745	0·5811524
2·8	1̄·9425997	1̄·8110100	1̄·6735631	1̄·5735667	1̄·4253428	1̄·3178449	0·5847512	0·5875643
2·9	1̄·9405497	1̄·8042604	1̄·6619047	1̄·5583370	1̄·4048177	1̄·2934823	0·5775580	0·5935051
3·0	1̄·9384997	1̄·7975108	1̄·6502462	1̄·5431072	1̄·3842959	1̄·2691196	0·5704933	0·5990105
3·1	1̄·9364497	1̄·7907612	1̄·6385877	1̄·5278774	1̄·3637724	1̄·2447569	0·5635547	0·6041158
3·2	1̄·9343997	1̄·7840115	1̄·6269293	1̄·5126477	1̄·3432590	1̄·2203942	0·5567408	0·6088483
3·3	1̄·9323497	1̄·7772619	1̄·6152708	1̄·4974179	1̄·3227255	1̄·1960316	0·5500483	0·6132386
3·4	1̄·9302997	1̄·7705122	1̄·6036124	1̄·4821882	1̄·3022020	1̄·1716689	0·5434768	0·6173097
3·5	1̄·9282496	1̄·7637626	1̄·5919539	1̄·4669584	1̄·2816785	1̄·1473062	0·5370234	0·6210834

* TABLE IV. will be found at p. 376.

TABLE VI.—Indices of Refraction calculated from the exponential law.

Formula $\mu = \dfrac{\lambda^n}{\dfrac{\lambda^n}{\epsilon_n} - a_n}$.

Media of each observer arranged in the order of agreement with the observed indices.

I. Fraunhofer's Observations.

Media	n.	μB.	μC.	μD.	μE.	μF.	μG.	μH.
Crown-glass No. 9 ...	1·9	1·525844	1·526852	1·529542	1·533024	1·536085	1·541629	1·546569
Water, 1 & 2, T.18°·75.	1·6	1·330989	1·331697	1·333540	1·335843	1·337807	1·341252	1·344221
Crown-glass M	2·0	1·554770	1·555933	1·559063	1·563160	1·566794	1·573463	1·579467
Sol. potash, T. 21°·5.	1·7	1.399639	1·400504	1·402777	1·405653	1·408131	1·412526	1·416357
Crown-glass No. 13...	1·9	1·524314	1·525306	1·527953	1·531382	1·534391	1·539846	1·544704
Oil of turpentine	2·1	1·470480	1·471553	1·474456	1·478303	1·481750	1·488134	1·493940
Flint-glass No. 13 ...	2·1	1·627729	1·629686	1·635000	1·642060	1·648408	1·660210	1·671017
Flint-glass No. 30 ...	2·2	1·623550	1·625451	1·630547	1·637391	1·643604	1·655276	1·666079
Flint-glass 23 (1&2)..	2·2	1·626571	1·628424	1·633533	1·640573	1·646877	1·658723	1·669683
Flint-glass No. 3......	2·2	1·602092	1·603775	1·608379	1·614559	1·620156	1·630686	1·640410

II. Rudberg's Observations.

Media	n.	μB.	μC.	μD.	μE.	μF.	μG.	μH.
Topaz, 2nd axis	1·7	1·608405	1·609285	1·611591	1·614518	1·617030	1·621493	1·625370
Quartz ex. ray........	1·7	1·549902	1·550822	1·553239	1·556300	1·558933	1·563608	1·567675
Arragonite, 1st axis...	1·9	1·527485	1·528207	1·530127	1·532614	1·534790	1·538731	1·542241
Quartz O. ray	1·7	1·540908	1·541804	1·544146	1·547111	1·549672	1·554199	1·558148
Topaz, 3rd axis	1·7	1·610521	1·611401	1·613706	1·616623	1·619130	1·623580	1·627457
Calc-spar O. ray	1·9	1·653084	1·654553	1·658471	1·663547	1·668019	1·676133	1·683372
Topaz, 1st axis	1·8	1·617927	1·618796	1·621088	1·624025	1·626574	1·631148	1·635168
Arragonite, 3rd axis..	1·9	1·676367	1·677763	1·681494	1·686332	1·690584	1·698302	1·705182
Calc-spar ex. ray......	2·1	1·483833	1·484483	1·486237	1·488558	1·490630	1·494464	1·497945
Arragonite, 2nd axis..	2·0	1·680675	1·682065	1·685810	1·690713	1·695067	1·703061	1·710266

III. Powell's Observations.

Media	n.	μB.	μC.	μD.	μE.	μF.	μG.	μH.
Sulphate of magnesia.	1·4	1·343422	1·344187	1·346148	1·348551	1·350559	1·354008	1·356922
Oil of anise, T.20°·9..	2·8	1·545105	1·547251	1·553423	1·562296	1·570857	1·588079	1·605172
Oil of anise, T.13°·2..	2·8	1·548273	1·550374	1·556375	1·565013	1·573344	1·590098	1·606710
Sol. of potash, T.16°..	1·4	1·402507	1·403496	1·406027	1·409125	1·411722	1·416183	1·419953
Sulphate of soda	1·6	1·339170	1·339902	1·341811	1·344198	1·346232	1·349805	1·352882
Nitrate of lead	1·8	1·345458	1·346195	1·348142	1·350631	1·352798	1·356678	1·360093
Superacetate of lead.	1·9	1·342956	1·343646	1·345491	1·347874	1·349964	1·353753	1·357118
Nitrate of bismuth ...	1·7	1·330709	1·331393	1·333189	1·335458	1·337412	1·340879	1·343892
Nitric acid	2·1	1·398821	1·399823	1·402537	1·406129	1·409351	1·415348	1·420740
Subacetate of lead ...	1·8	1·335024	1·335687	1·337440	1·339683	1·341632	1·345121	1·348190
Oil of sassafras	2·7	1·525786	1·527416	1·532054	1·538667	1·544915	1·557347	1·569485
Alcohol	1·7	1·362743	1·363443	1·365277	1·367599	1·369596	1·373138	1·376218
Muriate of lime	1·9	1·400651	1·401556	1·403968	1·407090	1·409835	1·414803	1·419233
Muriate of ammonia.	1·8	1·349979	1·350740	1·352739	1·355323	1·357559	1·361568	1·365098
Oil of anise, T. 15°·1..	2·8	1·548690	1·550828	1·556970	1·565803	1·574323	1·591457	1·608423
Muriatic acid	1·4	1·404948	1·406173	1·409243	1·413004	1·416155	1·421578	1·426167
Sulphuric acid........	1·9	1·432051	1·432967	1·435250	1·437921	1·440071	1·443612	1·446482
Rock-salt............	2·2	1·540232	1·541516	1·545027	1·549730	1·553990	1·561969	1·569323
Nitrate of mercury ...	1·8	1·340966	1·341733	1·343764	1·346359	1·348614	1·352660	1·356220
Creosote	2·5	1·531891	1·533613	1·533443	1·545145	1·551414	1·563568	1·575199
Pyroligneous acid ...	2·0	1·373285	1·374007	1·375950	1·378490	1·380744	1·384860	1·388566
Bisulphide of carbon.	2·5	1·618343	1·621648	1·630847	1·643690	1·655795	1·679461	1·702390
Water, T. 15°·8	2·0	1·331840	1·332489	1·334233	1·336515	1·338534	1·342229	1·345546
Oil of cassia, T. 14°..	3·5	1·594749	1·597716	1·606756	1·620875	1·635542	1·667645	1·702529
Solution of soda	1·6	1·403485	1·404500	1·407139	1·410441	1·413263	1·418217	1·422492
Muriate of baryta ...	2·6	1·340125	1·340651	1·342136	1·344210	1·346159	1·349984	1·353572
Nitrate of potash......	2·4	1·346201	1·346845	1·348638	1·351086	1·353342	1·357648	1·361690
Oil of cassia, T.22°·5..	3·4	1·590247	1·593184	1·602443	1·616743	1·631466	1·663353	1·697635
Oil of cassia, T. 10°..	3·2	1·596819	1·600068	1·609743	1·624364	1·639132	1·670436	1·703372

Oil of Anise further corrected for Temperature.

Media	n.	μB.	μC.	μD.	μE.	μF.	μG.	μH.
Oil of anise, T. 13°·25.	2·8	1·548290	1·550380	1·556382	1·565013	1·573410	1·590080	1·606673
Oil of anise, T. 15°·1..	2·8	1·548673	1·550814	1·556958	1·565798	1·574320	1·591476	1·608500
Oil of anise, T. 20°·9..	2·8	1·545120	1·547265	1·553430	1·562296	1·570850	1·588052	1·605122

Table VII.—Observed Indices of Refraction.
Media of each observer arranged in the order of agreement with the exponential law.

I. Fraunhofer's Observations.

Media	μB.	μC.	μD.	μE.	μF.	μG.	μH.
Crown-glass No. 9 ...	1·525832	1·526849	1·529587	1·533005	1·536052	1·541657	1·546566
Water, No. 2	1·330977	1·331709	1·333577	1·335849	1·337788	1·341261	1·344162
Crown-glass M	1·554774	1·555930	1·559075	1·563150	1·566741	1·573535	1·579470
Solution of potash ...	1·399629	1·400515	1·402805	1·405632	1·408082	1·412579	1·416368
Crown-glass No. 13...	1·524312	1·525299	1·527982	1·531372	1·534337	1·539908	1·544684
Water, No. 1	1·330938	1·331712	1·333577	1·335849	1·337818	1·341293	1·344177
Oil of turpentine......	1·470496	1·471530	1·474434	1·478353	1·481736	1·488198	1·493874
Flint-glass No. 13 ...	1·627751	1·629681	1·635036	1·642024	1·648260	1·660285	1·671062
Flint-glass No. 30 ...	1·623570	1·625477	1·630585	1·637356	1·643466	1·655406	1·666072
Flint-glass No.23 (2)..	1·626564	1·628451	1·633666	1·640544	1·646780	1·658849	1·669680
Flint-glass No. 3......	1·602042	1·603800	1·608494	1·614532	1·620042	1·630772	1·640373
Flint-glass No. 23(1)..	1·626596	1·628469	1·633667	1·640495	1·646756	1·658848	1·669686

II. Rudberg's Observations.

Media	μB.	μC.	μD.	μE.	μF.	μG.	μH.
Topaz, 2nd axis	1·6084	1·6093	1·6116	1·6145	1·6170	1·6215	1·6254
Quartz ex. ray	1·5499	1·5508	1·5533	1·5563	1·5589	1·5636	1·5677
Arragonite, 1st axis...	1·5275	1·5282	1·5301	1·5326	1·5348	1·5388	1·5422
Quartz O. ray	1·5409	1·5418	1·5442	1·5471	1·5496	1·5542	1·5582
Topaz, 3rd axis	1·6105	1·6114	1·6137	1·6167	1·6191	1·6236	1·6274
Calc-spar O. ray	1·6531	1·6545	1·6585	1·6636	1·6680	1·6762	1·6833
Topaz, 1st axis	1·6179	1·6188	1·6211	1·6241	1·6265	1·6312	1·6351
Arragonite, 3rd axis..	1·6763	1·6778	1·6816	1·6863	1·6905	1·6984	1·7051
Calc-spar ex. ray..	1·4839	1·4845	1·4863	1·4887	1·4907	1·4945	1·4978
Arragonite, 2nd axis ..	1·6806	1·6820	1·6859	1·6908	1·6951	1·7032	1·7101

III. Powell's Observations.

Media	μB.	μC.	μD.	μE.	μF.	μG.	μH.
Sulphate of magnesia.	1·3434	1·3442	1·3462	1·3486	1·3504	1·3540	1·3570
Oil of anise, T. 20°·9..	1·5451	1·5473	1·5533	1·5623	1·5707	1·5881	1·6053
Oil of anise, T.13°·25..	1·5482	1·5504	1·5565	1·5650	1·5733	1·5901	1·6066
Sol. of potash, T.16°..	1·4024	1·4036	1·4061	1·4091	1·4117	1·4162	1·4199
Sulphate of soda	1·3392	1·3398	1·3419	1·3442	1·3462	1·3499	1·3528
Nitrate of lead	1·3455	1·3461	1·3482	1·3506	1·3528	1·3568	1·3600
Superacetate of lead ..	1·3429	1·3437	1·3455	1·3480	1·3498	1·3538	1·3571
Nitrate of bismuth ...	1·3306	1·3315	1·3332	1·3355	1·3374	1·3410	1·3437
Nitric acid	1·3988	1·3998	1·4026	1·4062	1·4092	1·4155	1·4206
Subacetate of lead ...	1·3350	1·3357	1·3373	1·3398	1·3417	1·3453	1·3481
Oil of sassafras	1·5257	1·5275	1·5321	1·5387	1·5448	1·5575	1·5693
Alcohol	1·3628	1·3633	1·3654	1·3675	1·3696	1·3733	1·3761
Muriate of lime	1·4006	1·4016	1·4040	1·4070	1·4099	1·4150	1·4190
Muriate of ammonia..	1·3499	1·3508	1·3529	1·3552	1·3575	1·3617	1·3650
Oil of anise, T.15°·1..	1·5486	1·5508	1·5572	1·5659	1·5743	1·5912	1·6084
Muriatic acid	1·4050	1·4060	1·4095	1·4130	1·4160	1·4217	1·4261
Sulphuric acid	1·4321	1·4329	1·4351	1·4380	1·4400	1·4440	1·4463
Rock-salt..............	1·5403	1·5415	1·5448	1·5498	1·5541	1·5622	1·5691
Nitrate of mercury ...	1·3408	1·3419	1·3439	1·3462	1·3487	1·3528	1·3560
Creosote	1·5320	1·5335	1·5383	1·5452	1·5515	1·5639	1·5749
Pyroligneous acid ...	1·3729	1·3745	1·3760	1·3785	1·3807	1·3848	1·3884
Bisulphide of carbon..	1·6182	1·6219	1·6308	1·6438	1·6555	1·6799	1·7020
Water, T. 15°·8	1·3317	1·3326	1·3343	1·3364	1·3386	1·3429	1·3448
Oil of cassia, T. 14°..	1·5945	1·5979	1·6073	1·6207	1·6358	1·6671	1·7025
Solution of soda	1·4036	1·4039	1·4075	1·4109	1·4134	1·4181	1·4221
Muriate of baryta ...	1·3398	1·3406	1·3421	1·3438	1·3466	1·3504	1·3531
Nitrate of potash......	1·3457	1·3468	1·3487	1·3510	1·3533	1·3586	1·3608
Oil of cassia, T.22°·5..	1·5895	1·5930	1·6026	1·6174	1·6314	1·6625	1·6985
Oil of cassia, T. 10°..	1·5963	1·6007	1·6104	1·6249	1·6389	1·6698	1·7039

TABLE IV.—The Extrusions. (Media in order of value of X.)   I. Regular Media.

Media.	$b_{x'}$	$c_{x'}$	$d_{x'}$	$e_{x'}$	$f_{x'}$	$g_{x'}$	$h_{x'}$	X.
	−0·000059	−0·000030	+0·000025	+0·000054	+0·000122	−0·000002	−0·000110	+0·000201
Sulphate of magnesia	59	30	25	54	122	2	110	201
Solution of potash, T. 16°	27	76	27	96	79	12	87	202
Water, No. 1, T. 18°·75	79	45	54	108	88	17	109	250
Sulphate of soda	102	6	31	112	109	36	108	252
Water, No. 2, T. 18°·75	90	35	57	104	95	15	116	256
Topaz, 3rd axis	96	34	75	86	100	12	119	261
Topaz, 2nd axis	105	41	72	118	101	9	136	291
Nitrate of bismuth	54	94	74	109	113	55	93	296
Quartz ex. ray	118	30	63	127	115	5	152	305
Topaz, 1st axis	110	41	80	105	126	22	138	311
Quartz O. ray	115	39	64	130	123	7	156	318
Arragonite, 1st axis	136	42	100	141	114	25	152	355
Subacetate of lead	127	55	144	96	116	59	115	356
Calc-spar ex. ray	153	31	106	136	129	17	170	371
Nitrate of lead	172	4	86	171	122	49	154	379
Solution of potash, T. 21°·5	134	52	84	158	139	24	171	381
Nitrate of mercury	57	137	45	233	104	60	128	382
Muriate of ammonia	105	82	37	212	156	56	162	405
Superacetate of lead	128	81	84	119	207	26	198	433
Water, T. 15°·8	83	109	107	232	132	253	3	448
Crown-glass No. 13	178	58	114	192	176	26	220	482
Crown-glass No. 9	177	61	110	200	172	17	227	482
Solution of muriate of lime	165	86	120	240	147	74	182	507
Arragonite, 3rd axis	184	83	112	232	205	35	247	549
Arragonite, 2nd axis	215	62	133	230	207	36	257	570
Calc-spar O. ray	229	57	147	229	206	28	268	582
Nitrate of potash	98	166	107	284	224	316	38	618
Crown-glass M	232	77	154	247	66	28	288	625
Muriate of baryta	180	147	135	430	245	147	157	631
Oil of turpentine	272	81	193	269	301	32	322	707
Nitric acid	261	82	161	268	260	74	313	730
Rock-salt	355	106	309	325	391	71	362	894
Flint-glass No. 3	374	145	232	432	397	37	499	1055
Flint-glass No. 13	404	135	262	431	418	36	515	1090
Flint-glass No. 23 (2)	429	149	280	469	422	44	545	1167
Flint-glass No. 30	419	159	277	468	421	47	542	1167
Flint-glass No. 23 (1)	437	151	282	485	528	48	552	1188
Creosote	637	164	453	624	668	98	706	1605
Oil of sassafras	629	275	435	709	877	54	854	1812
Oil of anise, T. 13°·25	874	344	561	994	941	16	1198	2432
Oil of anise, T. 20°·9	939	366	630	1025		13	1278	2596

TABLE VIII.—Differences between Observed and Calculated Indices.

Media.	μB.	μC.	μD.	μE.	μF.	μG.	μH.	Sum.
F. Crown-glass No. 9	+0·000012	+0·000003	−0·000045	+0·000019	+0·000033	−0·000028	+0·000003	0·000143
F. Water, No. 2	−12	+12	−37	+6	+19	−72	+59	154
F. Crown-glass M	−4	+3	−12	+10	+53	−53	+3	157
F. Solution of potash	+10	−11	−28	+21	+49	−62	−11	183
F. Crown-glass No. 13	+2	+7	−29	+10	−54	−41	−20	184
F. Water, No. 1	−51	−15	−37	−6	+11	−64	+44	205
F. Oil of turpentine	−16	+23	+22	+50	−14	−75	+66	255
F. Flint-glass No. 13	−22	+5	−36	+36	+148	−130	+45	367
F. Flint-glass No. 30	+20	−26	−38	+35	+138	−126	−7	394
F. Flint-glass No. 23 (2)	+7	−27	−133	+29	+97	−86	−3	422
F. Flint-glass No. 3	−50	−25	−115	+27	+114	−125	−37	454
F. Flint-glass No. 23 (1)	+25	−45	−134	+78	+121	−17	−3	531
R. Topaz, 2nd axis	−5	+15	+19	+18	+30	−8	+30	134
R. Quartz ex. ray	+2	+22	−61	.....	+33	+69	−25	151
R. Arragonite, 1st axis	−15	+7	−27	+14	+10	−1	+41	183
R. Quartz O. ray	+8	+4	−54	+11	−72	−20	+52	202
R. Topaz, 3rd axis	+21	+1	−6	+77	+30	−67	+57	212
R. Calc-spar O. ray	−16	+53	−29	−53	+19	−52	+72	309
R. Topaz, 1st axis	+27	+4	−12	+75	+74	−98	+68	312
R. Arragonite, 3rd axis	+67	+37	−106	−32	+84	−36	+82	506
R. Calc-spar ex. ray	+67	+17	−63	+142	+70	−139	+145	540
R. Arragonite, 2nd axis	+75	+65	−90	+87	+33	+8	+166	655
P. Sulphate of magnesia	+22	+13	−52	−49	+159	−21	−78	381
P. Oil of anise, T. 20°·9	+5	+49	+23	+4	+157	+48	−128	387
P. Do. cor. for temp.	+20	+35	+30	+4	+150	+2	−178	465
P. Oil of anise, T. 13°·25	+73	+26	+125	+13	+44		+110	393

Media								Sum
Sulphuric acid	−93	−38	+67	+106	+85	+7	−120	nil 258
Muriatic acid	−117	−40	+83	+124	+103	+8	−145	310
Alcohol	−134	−44	+93	+143	+116	+9	−165	352
Solution of soda	−183	−63	+126	+198	+163	+9	−232	487
Pyroligneous acid	−929	−341	+618	+1016	+885	+10	−1239	2519
Oil of anise, T. 11°·1	−1796	−680	+1172	+1981	+1762	+23	−2462	4938
Oil of cassia, T. 10°	−1959	−754	+1265	+2171	+1951	+48	−2722	5435
Oil of cassia, T. 22°·5	−2016	−781	+1297	+2235	+2023	+62	−2820	5617
Oil of cassia, T. 14°								

* The extrusions for these last nine, as deduced from observation, may be found by comparing Tables II. and III.

Table VIII.—Differences between Observed and Calculated Indices (*continued*).

Media.	μB.	μC.	μD.	μE.	μF.	μG.	μH.	Sum.
P. Oil of anise, cor. for T.	+0·000090	+0·000020	+0·000118	+0·000013	+0·000010	−0·000020	+0·000073	0·000444
P. Sol. of potash, T. 16°.	+107	−104	−73	+25	−22	−17	+53	401
P. Sulphate of soda	−30	+102	−89	−2	+32	−95	+82	432
P. Nitrate of lead	+42	+95	−58	+31	−2	−122	+93	443
P. Superacetate of lead	+56	−54	+9	−126	+164	−47	+18	474
P. Nitrate of bismuth	+109	+107	+11	−42	+12	−121	+192	594
P. Nitric acid	+21	−23	+63	−71	+151	−152	+140	621
P. Subacetate of lead	+24	+13	−140	−117	−68	−179	+90	631
P. Oil of sassafras	−86	+84	+46	−33	+115	−153	+185	702
P. Alcohol	+57	−143	−123	+99	−4	−162	+118	706
P. Muriate of lime	+51	+44	−32	+90	+65	−197	+233	712
P. Muriate of ammonia	+79	+60	−161	+123	+59	−142	+98	722
P. Oil of anise, T. 15°·1	+90	+28	−230	+97	+23	−257	+23	748
P. Do. cor. for temp.	+73	+14	−242	−102	+20	−276	+100	827
P. Muriatic acid	+52	+173	+257	−4	−155	+129	+67	837
P. Sulphuric acid	−49	−67	+150	+79	−71	+388	+182	986
P. Rock-salt	−132	+16	−227	+70	−110	+231	+223	1009
P. Nitrate of mercury	+166	−167	+136	−159	−86	−140	+220	1074
P. Creosote	+109	−113	−143	−55	−86	−332	+299	1137
P. Pyroligneous acid	+385	+493	−50	+10	+44	−60	+166	1208
P. Bisulphide of carbon	+143	+252	+47	−110	+295	+439	+390	1676
P. Water, T. 15°·8	+140	+111	−67	+115	+66	+671	+746	1916
P. Oil of cassia, T. 14°	+249	+184	+544	−175	−258	+545	+29	1984
P. Solution of soda	+115	+600	+361	−459	+137	+117	+392	2181
P. Muriate of baryta	+325	+51	+36	+410	+441	+454	+472	2189
P. Nitrate of potash	+501	+45	+62	−86	+42	+952	+890	2578
P. Oil of cassia, T. 22°·5	+747	+184	−157	+657	+66	+853	−865	3529
P. Oil of cassia, T. 10°	+519	+632	−667	+536	+232	+636	−528	3750

Table IX.—Differences between Observed and Calculated Indices of C, D, E, and G under M. Cauchy's law, B, F, and H being assumed. Also differences between these errors and those arising under exponential law S_n.

Media.	μC.	μD.	μE.	μG.	Sum.	μE.	μD.	μG.	μF.	Sum.	S_n.	Diff. +	D iff. −
F. Crown glass No. 9	0·000000	−0·000200	−0·000200	+0·000200	0·000600	−0·000200	−0·000200	+0·000200	+0·000010	0·000600	0·000143	0·000457	
F. Water, No. 2	+100	−200	−100	+100	500	−100	−200	+100		500	154	347	
F. Crown-glass M	+100	−100	−100	+200	500	−100	−100	+200		500	157	343	
F. Solution of potash	......	−100	−200	+100	400	−200	−100	+100		400	183	217	
F. Crown-glass No. 13	......	−100	−200	+100	400	−200	−100	+100		400	184	216	
F. Water, No. 1	−100	−200	−100	+100	200	−100	−200	+100		200	205	295	0·000055

Substance												
F. Flint-glass No. 13	−			100	−	400	+	300	800	307	433	
F. Flint-glass No. 30	−	100		100	−	200	+	300	700	394	306	
F. Flint-glass No. 23 (2)				200	−	300	+	300	800	422	378	
F. Flint-glass No. 3		100		300	−	300	+	200	900	454	446	
F. Flint-glass No. 23 (1)				100	−	300	+	300	800	531	269	
R. Topaz, 2nd axis	+			100	−	100	−	200	800	134	266	12
R. Quartz ex. ray				100	−	100	+	200	400	151	349	106
R. Arragonite, 1st axis	+	100	−	100	+	100	−	100	500	183	117	140
R. Quartz O. ray		100			−	100	+	200	300	202	198	355
R. Topaz, 3rd axis				100	−	300	+	200	400	212	388	
R. Calc-spar O. ray	+	100	+	100	−	300	+	100	600	309	191	
R. Topaz, 1st axis		100			−	200	−		500	312		
R. Arragonite, 3rd axis		100	−	200	−	200	−	100	300	506		
R. Calc-spar ex. ray	−				−	200	+	100	400	540	419	
R. Arragonite, 2nd axis		200	−	100	−	300	+		400	655	813	
P. Sulphate of magnesia			−	400	−		+	200	300	381	707	
P. Oil of anise, T. 20°.9	+	300	+	200	−	300	+	500	800	387	2099	
P. Oil of anise, T. 13°.25		300	+	200	−	600	+	600	1200	393	168	
P. Solution of potash	−	500	+	100	−	400	−	1000	1100	401	57	21
P. Sulphate of soda		100	−		−		−	200	2500	432	526	102
P. Nitrate of lead	+	400	+	300	+		+	100	600	443	806	206
P. Superacetate of lead	−	100	+	300	−	400	−	200	500	474		712
P. Nitrate of bismuth		200	+	500	−	300	+	300	1000	594		
P. Nitric acid	+		−	100	−	200	+	200	1400	621	469	
P. Subacetate of lead	−	300	+	400	−	300	−	300	600	631		
P. Oil of sassafras			−	100	+	300	−	100	1100	702		
P. Alcohol	+	200	−	200	−		+	100	600	706		
P. Muriate of lime			−		−	100	−		500	712	78	386
P. Muriate of ammonia	+	200	+	100	+	600	+	300	800	722	1052	309
P. Oil of anise, T. 15°.1	−	300	−	200	−	400	−	1000	1800	748	263	274
P. Muriatic acid	+	400	−	500	+	300	+		1100	837		137
P. Sulphuric acid		700	+		−	100	+	300	600	986		8
P. Rock-salt	−	800	−	400	−	200	−		700	1009		
P. Nitrate of mercury	+	200	+	200	+	200	+	200	800	1074		
P. Creosote		400	−	400	−	2000	+	100	1000	1137		
P. Pyroligneous acid	−	700	+	200	−	100	−	4400	1200	1208		
P. Bisulphide of carbon		100	+	1700	+	500	+	500	8900	1676	7224	1116
P. Water, T. 15°.8	+	300	−		−	400	+	2400	800	1916		
P. Oil of cassia, T. 14°		400	−	700	−	700	−	600	4000	1984	2016	
P. Solution of soda	+	700	+	300	+		−	100	2000	2181		
P. Muriate of baryta	+	100	+	200	−	1800	+	1100	1100	2189		
P. Nitrate of potash		300	−	200	−	1700	+	2900	1600	2578	1971	181
P. Oil of cassia, T. 22°.5	+	400	+	400	+			2300	5500	3529	1971	1089
P. Oil of cassia, T. 10°	−	1100	−	1200	−		+		6300	3750	2550	978

LI. *Chemical Notices from Foreign Journals.* By E. ATKIN-
SON, *Ph.D., F.C.S., Teacher of Physical Science in Cheltenham
College.*

[Continued from p. 287.]

DEBRAY has described * some methods for obtaining a
number of artificial phosphates and arseniates, many of
them identical in form and composition with known minerals.

When an insoluble carbonate is treated with excess of aqueous
phosphoric acid, or arsenic acid, the carbonate is usually changed
into a crystallized phosphate or arseniate. The proportion of
water of hydration varies with the temperature. With phos-
phoric acid and carbonate of lime, a phosphate of lime is ob-
tained of the composition $PO^5, 2CaO, HO + 4HO$. At a tem-
perature of 100° the phosphate $PO^5, 2CaO, HO$ is obtained.

Arsenic acid and carbonate of lime at the ordinary tempera-
ture give the body $AsO^5, 2CaO, 4HO$, which is the mineral
Haidingerite.

In a similar manner the following bodies have been obtained :—

At 70° phosphate of zinc . . . $PO^5, 3ZnO, 4HO$
 ,, 250° (in a sealed tube) . . . $PO^5, 3ZnO, HO$
 ,, 70° phosphate of copper . . $PO^5, 3CuO, 3HO$
 ,, 70° arseniate of copper . . $AsO^5, 2CuO, HO, 3HO$
 ,, 70° phosphate of manganese $PO^5, 3MnO, 4HO$.

By acting on the insoluble phosphates of lime, magnesia, &c.
with a metallic nitrate or sulphate, crystallized compounds are
obtained, the composition of which varies with the temperature.
At 70° phosphate of lime, $PO^5, 2CaO, HO$, and arseniate of lime,
$AsO^5, 2CaO, HO$, give with nitrate of copper the phosphate
$PO^5, 3CuO, 3HO$, and the arseniate $AsO^5, 3CuO, 4HO$; and
at a temperature a little above 100° C.,

The phosphate of copper, $PO^5, 4CuO, HO$ (libethenite);

The arseniate of copper, $AsO^5, 4CuO, HO$ (olivenite).

These latter bodies, which have the form as well as the com-
position of the native minerals, are better obtained when the
reaction is effected in a sealed tube at 140°—150°.

At high temperatures (250°—270°) water transforms certain
phosphates and arseniates into other well-crystallized species.
Blue phosphate of copper, $PO^5, 3CuO, 3HO$, is transformed
into magnificent crystals of libethenite ; and arseniate of copper,
$AsO^5, 3CuO, 4HO$, is changed into olivenite.

Crystallized double phosphates are obtained by mixing the
solutions of acid phosphates with metallic solutions. A solution
of acid phosphate of lime, mixed with a solution of nitrate of

* *Bulletin de la Société Chimique,* p. 134.

uranium, yields *chalcolite*, PO^5, $2CuO + PO^5 (Ur^2 O^3)^4 + 16HO$, or double phosphate of copper and uranium.

Chancel proposes * the following method of separating and estimating phosphoric acid in the presence of bases : it depends on the insolubility of phosphate of silver, $3AgO$, PO^5, in a neutral liquid.

The substance taken is dissolved in the least quantity of dilute nitric acid, and the solution diluted with water. The clear liquid is mixed with a sufficient quantity of nitrate of silver, and then with a slight excess of carbonate of silver. This operation must be performed in the cold when the liquid contains any substance, such as manganese, which is precipitated by the application of heat.

The phosphoric acid soon separates as yellow phosphate of silver ; the precipitation is complete when the liquid no longer reddens blue litmus paper ; the precipitate is then filtered, carefully washed, introduced into a flask, and dissolved in nitric acid. The silver is now precipitated by means of hydrochloric acid and the filtered liquid supersaturated with ammonia, and the ammoniacal phosphate precipitated with sulphate of magnesia in the ordinary manner.

The advantages which silver salts have in this separation of phosphoric acid, consist greatly in the readiness with which the silver is removed from the solution ; but they have the great disadvantage that, when the phosphoric acid is in combination with alumina, oxide of iron or chrome, it is not separated from these bodies, but merely precipitated with them. The use of bismuth compounds which Chancel proposes† is not amenable to these objections ; it furnishes an exact and easy method, one susceptible of the widest application.

The method depends on the insolubility of phosphate of bismuth in liquids which contain even a considerable proportion of free nitric acid. The acid nitrate of bismuth is prepared by dissolving one part of pure subnitrate of bismuth $(BiO^3, NO^5 + Aq)$ in four parts of nitric acid of sp. gr. 1·36., and adding to the solution thirty parts of water. Thus prepared, the solution is not made turbid either by boiling or by the addition of any quantity of water. The substance to be determined is dissolved in distilled water ; if necessary a small quantity of nitric acid is added, care being taken to avoid excess, and the solution diluted with water. If the solution contains sulphates or chlorides they must be removed, the former by adding nitrate of baryta, and the latter by nitrate of silver. The solution of acid nitrate of bismuth is then added, as long as a

precipitate is formed. The precipitate which separates is of a beautiful white, is very dense, and settles readily, more especially when warmed; the whole liquid is heated to boiling, filtered and washed with boiling water. It is then dried and heated. It has the composition BiO^3, PO^5, which is that of a neutral phosphate, for bismuth is triatomic, and replaces three atoms of water in tribasic phosphoric acid.

Pyrophosphoric acid is also precipitated by acid nitrate of bismuth as neutral pyrophosphate of bismuth, $2BiO^3$, $3pPO^5*$. The precipitate is white, but much more voluminous than with tribasic phosphoric acid. Metaphosphoric acid is likewise precipitated by nitrate of bismuth. Both these precipitates, however, are converted into the tribasic phosphate of bismuth when boiled with excess of the bismuth solution; the metaphosphate requires a somewhat more prolonged ebullition. It is therefore of little importance in what modification the phosphoric acid exists in the solution to be determined.

This method affords a very delicate means of determining phosphoric acid. Chancel was able to determine and separate a milligramme of phosphoric acid in the presence of 120 milligrammes of alumina; and the precipitation is so rapid that it will be possible to estimate phosphoric acid by means of a standard solution of acid nitrate of bismuth.

To estimate magnesia in the presence of the alkalies, Scheerer† proceeds as follows :—

The mixture of magnesia and the alkalies being given, is converted into neutral sulphates and weighed. This mass is dissolved in a small quantity of water, and the solution divided into two parts of known weight. In the one the magnesia is determined by the addition of phosphate of soda, and in the other the potass is precipitated by bichloride of platinum. The soda is estimated from the difference.

According to Rose ‡, when silica of the density 2·2 is mixed with fluoride of ammonium and the mixture heated, the silica is completely volatilized as fluoride of silicon. With quartz and sand the action is less energetic. Rose recommends the use of fluoride of ammonium as convenient for decomposing silicates. It may readily be prepared by adding ammonia in excess to the commercial acid, and then a little carbonate and sulphide of ammonium. The liquid is allowed to stand, the clear liquid

* Chancel represents ordinary or tribasic phosphoric acid as PO^5, bibasic phosphoric or pyrophosphoric acid as pPO^5, and monobasic or metaphosphoric acid as mPO^5.

† Liebig's *Annalen*, November 1859.

‡ Poggendorff's *Annalen*, September 1859.

decanted and evaporated to dryness in a platinum crucible ; a small quantity of carbonate of ammonia is added occasionally during the evaporation, and when the mass becomes pasty, it is stirred with a platinum spatula. The dried salt may be kept in vessels of platinum, silver, or gutta percha.

In order to decompose a silicate, it is mixed in a state of fine powder with six times its weight of fluoride of ammonium, a small quantity of water added, and the mixture heated at first gently, and gradually to redness as long as vapours are given off. Usually one operation is sufficient ; the residue is treated with sulphuric acid, and the excess of this acid driven off. Where the sulphates formed do not completely dissolve in water containing a little hydrochloric acid but leave a residue, the insoluble part is again treated with fluoride of ammonium.

The temperature must not be raised too high in the operation ; for if the silicate contained alumina a fluoride of aluminum might be formed, difficult to be decomposed by sulphuric acid.

Hyposulphite of soda, according to Löwe[*], dissolves sulphate of lead, which may in this way be separated from sulphate of baryta. A concentrated solution of hyposulphite is poured on the mixture of the two salts, and the whole digested between 15° and 20°. At a higher temperature a sulphide of lead insoluble in hyposulphite of soda might be formed. The residual sulphate of baryta carefully washed is weighed, and as a control the lead in the hyposulphite may also be determined.

For the preparation of permanganate of potash, Béchamp [†] recommends the following method. In an iron basin ten parts of powdered binoxide of manganese are mixed with twelve parts of fused caustic potash ; a little water is added to the mixture, which is rapidly dried, and introduced while still hot into a tubulated stoneware retort, and a current of pure dry oxygen passed into it. To the neck of the retort a tube is fitted just dipping under mercury. The absorption of oxygen is very rapid ; it is complete when it bubbles through the mercury. The cooled mass is then exhausted with hot water, and a current of carbonic acid passed through the solution, by which the manganate is transformed into permanganate. When the solution has the characteristic colour of the permanganate the current of gas is stopped, the oxides of manganese are allowed to settle, the clear solution rapidly evaporated and allowed to crystallize ; the mother-liquors yield a further crop of crystals. In general a pound of binoxide gives five to six ounces of permanganate at the first crystallization.

[*] *Journal für Praktische Chemie,* vol. lxxvii. p. 75.
[†] *Annales de Chimie et de Physique,* November 1859.

Lactic acid is regarded by Kolbe as oxypropionic acid, that is, as propionic acid in which an atom of hydrogen in the radical is replaced by peroxide of hydrogen, just as chloropropionic acid is propionic acid containing an atom of chlorine in the place of an atom of hydrogen.

$$\left.\begin{array}{c} C^6\,H^5\,O^2 \\ H \end{array}\right\}O^2 \qquad \left.\begin{array}{c} C^6\,H^4\,Cl\,O^2 \\ H \end{array}\right\}O^2 \qquad \left.\begin{array}{c} C^6\,H^4\,(HO^2)\,O^2 \\ H \end{array}\right\}O^2.$$
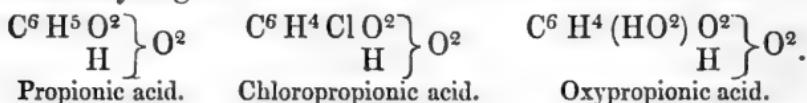
Propionic acid.   Chloropropionic acid.   Oxypropionic acid.

In accordance with this view it might be expected that if hydrogen could be directly substituted for the group HO^2, propionic acid would be regenerated, and this mode of regarding lactic acid would receive additional support. Lautemann * has made a series of experiments in this direction.

The change is not effected by treating lactic acid with sodium, or with sodium amalgam, nor with electrolytic hydrogen; but it takes place by means of hydriodic acid. When lactic acid was saturated with hydriodic acid gas, a separation of iodine commenced almost immediately; to complete the change, the solution was heated in a closed tube to 140°. The contents of the tube were then distilled with sulphuric acid; the distillate, which contained hydriodic acid and free iodine, was treated with carbonate of silver, and filtered. On cooling, crystals separated which had the appearance and all the properties of propionate of silver. The change is thus expressed:

$$C^6\,H^6\,O^6 + 2HI = C^6\,H^6\,O^4 + 2HO + 2I$$
Lactic acid.   Propionic acid.
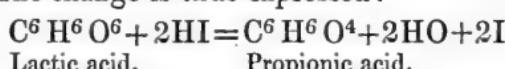
Regarding alanine as amidopropionic acid, $\left.\begin{array}{c} C^6\,H^4\,(NH^2)O^2 \\ H \end{array}\right\}O^2$,

or propionic acid in which an atom of hydrogen in the radical is replaced by the group NH^2, a view rendered highly probable by the investigations of Perkins, Cahours and Ulrich, and regarding lactic acid as oxypropionic acid,—it is obvious to expect that lactic acid may be transformed into alanine, as Strecker has shown that alanine may be transformed into lactic acid. Kolbe † has effected this in the following manner :—By treatment with pentachloride of phosphorus, lactic acid is changed into chloride of chloropropionyle; by treating this with absolute alcohol, it is changed into chloropropionic ether. This is then heated with ammonia in a closed vessel to 100°, the contents of the vessel evaporated and exhausted with absolute alcohol, by which the greater part of the chloride of ammonium is left undissolved. It is then boiled with water to expel the alcohol, and subsequently with oxide of lead, which decomposes the rest of the chloride of ammonium, forming with the chlorine a basic chloride of lead,

while the ammonia is liberated as gas. The mixture is then filtered, saturated with sulphuretted hydrogen, filtered and evaporated; on cooling, alanine is obtained in beautiful crystals.

$$\left.\begin{array}{c}C^6H^4ClO^2\\C^4H^5\end{array}\right\}O^2+2NH^3+2HO=\left.\begin{array}{c}C^6H^4(NH^2)O^2\\H\end{array}\right\}O^2+C^4H^6O^2+NH^4Cl$$

Chloropropionic ether.	Amidopropionic acid.	Alcohol. Chloride o ammonium
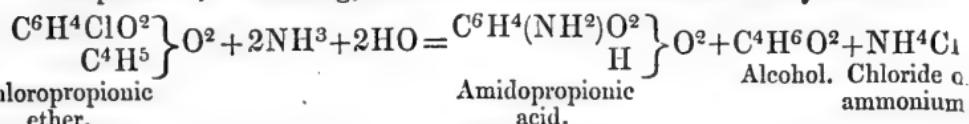
According to Lautemann[*], the following is an advantageous modification of Bensch's method of preparing lactic acid. The proportions of sugar, tartaric acid, milk, and cheese are the same; but instead of chalk an equivalent quantity of oxide of zinc (commercial zinc-white) is employed. The temperature must be between 40—50°; the fermentation is complete in eight or ten days. The whole mixture is boiled in a copper vessel, filtered, evaporated, again filtered, and allowed to stand. On cooling, pure lactate of zinc separates. To obtain the free acid the lactate is dissolved in boiling water, the zinc separated by sulphuretted hydrogen, filtered and concentrated. This solution contains mannite and lactic acid; to separate the two it is agitated with ether, which dissolves the lactic acid, and leaves the mannite. On evaporating the etherial solution the lactic acid is left behind.

Kekulé found[†] that chloracetate of potass, when heated in the presence of water, is decomposed with the assimilation of two equivalents of water, forming chloride of potassium and glycolic acid :—

$$C^4H^2ClKO^4+2HO=KCl+C^4H^4O^6.$$

Chloracetate of potass.	Glycolic acid.
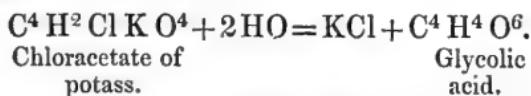
Heinz has found[‡] that when the same salt is heated in methylic alcohol, or, still better, when free chloracetic acid is treated with methylate of soda, chloride of sodium and a new acid are formed :—

$$C^4H^3ClO^4+C^2H^3NaO^2=NaCl+C^6H^6O^6.$$

Chloracetic acid.	Methylate of soda.	New acid.
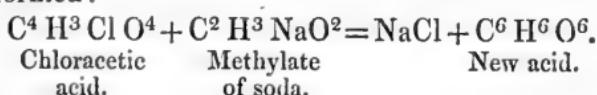
This experiment was undertaken in the expectation of forming lactic acid. The new acid is, however, isomeric, and not identical with lactic acid. The reaction is capable of great extension; by acting on the homologues of methylic alcohol, a series of new acids is obtained corresponding to the higher members of the lactic acid series.

* Liebig's *Annalen*, February 1860.
† Phil. Mag. vol. xvi. p. 138.
‡ Poggendorff's *Annalen*, Feb. 1860.

The monochloracetic acid was prepared by Hoffmann's method, which was found very convenient. A quantity of sodium was then dissolved in anhydrous methylic alcohol, and a corresponding quantity of chloracetic acid added. The action was very energetic, each addition of the acid being attended with a hissing sound. When the action was complete, the mixture was heated for some hours to a temperature of 100°; the alcohol was then distilled off, hydrochloric acid added in slight excess, the mixture rendered feebly alkaline with soda, and evaporated to dryness. This mass was exhausted with alcohol, the alcohol distilled off, and the aqueous solution of the residue mixed with sulphate of zinc and evaporated to dryness. On treating this mass with alcohol a zinc-salt was dissolved out, which was deposited from the alcoholic solution in large, colourless, acute rhombic octahedra. The analyses gave for the formula of this salt, dried in the air, $C^6 H^5 ZnO^6 + 2HO$: the water is given off at 100° C.

To obtain the free acid from this salt, it was dissolved in water, the solution saturated with sulphuretted hydrogen, and filtered from the sulphide of zinc; on evaporating the filtrate, acid vapours were given off; it was accordingly distilled. The temperature gradually rose to 198°, at which point it remained constant. What now passed over consisted of the free acid, which is a colourless viscid liquid of the sp. gr. 1·18. It has an acid, but not unpleasant taste; its boiling-point is 198°; it is very soluble in water, and readily attracts moisture from the atmosphere. It burns, when it is inflamed, with a blue non-fuliginous flame. By saturating the acid with carbonate of zinc, filtering and evaporating, a zinc-salt was obtained identical with that obtained at first.

The acid, though isomeric with lactic acid, differs widely from it, and also from sarcolactic acid. The new acid is volatile; its zinc-salt crystallizes readily in large crystals, and is very soluble. Its potash and baryta salts readily crystallize; its lime-salt does not crystallize, and is easily soluble in water; and lastly, the silver salt melts at 100°, and does not fuse. In all these points there is a wide difference from the corresponding compounds of the lactic acids.

The acid is derived from glycolic acid by the substitution of methyle. Glycolic acid is bibasic, and, according to Socoloff and Strecker, contains two atoms of replaceable hydrogen, one of which is more readily replaced by a metal, and the other by an acid radical. If this acid were glycolic acid in which the basic hydrogen is replaced by methyle, it would have a constitution analogous to that of the ordinary ethers; it would be, in fact, a glycolate of methyle, and would yield, when treated with an alkali, an alkaline glycolate and free methylic alcohol. But

Heinz found that, when treated with caustic soda, it yielded a soda salt which was not glycolate of soda. Another supposition is that it is glycolic acid in which the atom of hydrogen more readily replaced by an acid radical is replaced by methyle; on which view its constitution would be represented thus, $\left.\begin{array}{c} C^4H^2O^2 \\ C^2H^3, H \end{array}\right\} O^4$.

Heinz found that the acid, when treated with benzoic acid, did not form benzoglycolic acid, as might have been expected on this view.

Heinz considers that the acid is oxacetic acid (glycolic acid) in which an atom of methyle is contained in the radical. He writes the formula thus, $\left.\begin{array}{c} C^6\,H^5\,O^4 \\ H \end{array}\right\} O^2$, and names it *methoxacetic acid* to express this mode of deriving it. The homologous acids, formed by the same reaction, receive the names *ethoxacetic acid, amoxacetic acid,* &c.

Heinz has described several of the salts. *Methoxacetate of ammonia* forms a mass of radiating crystals which are very deliquescent. *Methoxacetate of baryta,* BaO $C^6\,H^5\,O^5$, forms colourless, transparent, prismatic crystals, readily soluble in water, but difficultly so in alcohol. *Methoxacetate of copper,* CuO $C^6\,H^5\,O^5 + 2$HO, forms bluish-green, transparent, acute rhombic prisms with good reflecting surfaces. They are perfectly soluble in water, but little so in alcohol. *Methoxacetate of lime* forms a syrupy solution which does not crystallize; but if evaporated over sulphuric acid, it dries up to a solid mass, which gradually becomes crystalline. *Methoxacetate of potash,* KO $C^6\,H^5\,O^5 + 8$HO, forms large colourless transparent prisms permanent in the air. They are readily soluble in alcohol. On the addition of ether to this solution, a salt of the formula KO $C^6\,H^5\,O^5 + 6$HO is precipitated. *Methoxacetate of lead,* PbO $C^6\,H^5\,O^5$, forms a solid, white, crystalline mass like Wavellite. *Methoxacetate of silver,* AgO $C^6\,H^5\,O^5$, forms fine needles, which are somewhat soluble in cold and readily so in hot water.

Ethoxacetic acid, $C^8\,H^8\,O^6 = \left.\begin{array}{c} C^8\,H^7\,O^2 \\ H \end{array}\right\} O^4$, is obtained in a similar manner to methoxacetic acid. It boils at about 190° C. The baryta salt is very readily soluble in alcohol and ether. Dissolved in water and evaporated, it forms a syrupy solution, which crystallizes, after some time, in small needles.

Amoxacetic acid, $C^{14}\,H^{14}\,O^6 = \left.\begin{array}{c} C^{14}\,H^{13}\,O^2 \\ H \end{array}\right\} O^4$, is similarly obtained by the action of chloracetic acid on amylate of soda. A solution of its zinc-salt in dilute alcohol is decomposed by sulphuretted hydrogen and distilled. At first alcohol and water pass over, and then the acid at a temperature of about 235—

240° C. It forms a yellowish-green liquid, somewhat insoluble in water, but miscible in every proportion with alcohol and ether. Heated in a platinum spoon it takes fire, and burns with a clear flame.

Neither the zinc nor the baryta salts crystallize. The potash salt crystallizes in a wavellite-like mass. The copper salt forms microscopic blue needles.

By the action of chloracetic acid on the soda compound of phenylic acid, Heinz obtained a new acid, phenoxacetic acid,

$$C^{12} H^5 NaO^2 + C^4 H^3 ClO^4 = NaCl + C^{16} H^8 O^6.$$

$$\underset{\substack{\text{Phenylate} \\ \text{of soda.}}}{} \qquad \underset{\substack{\text{Chloracetic} \\ \text{acid.}}}{} \qquad \underset{\text{New acid.}}{}$$
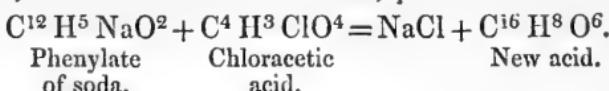
The new acid is isomeric with the next higher homologue of salicylic acid, $C^{14} H^6 O^6$, or, as is more probable, with Gerland's oxybenzoic acid.

Phenoxacetate of baryta, $C^{16} H^7 BaO^6 + 3HO$, is obtained by saturating the free acid with baryta water. It crystallizes in very large, thin laminæ. *Phenoxacetate of copper,* $C^{16} H^7 CuO^6 + 2HO$, forms small tabular or prismatic cærulean crystals, which are very little soluble in water. *Phenoxacetate of lead* forms small microscopic granules. *Phenoxacetate of silver,* $C^{16} H^7 AgO^6$, forms small, flat prismatic crystals, which are frequently arranged in groups.

Phenoxacetic acid, $C^{16} H^8 O^6 = \left. \begin{matrix} C^{16} H^7 O^4 \\ H \end{matrix} \right\} O^2$. The free acid may be obtained from any of its soluble salts by the addition of a strong mineral acid. According as the decomposition is effected in the warm or in the cold, the acid is precipitated as an oil, or as an amorphous powder, which by agitation becomes crystalline. In the temperature of the water-bath it gradually disappears.

The brown liquid produced by the direct action of chlorine on sulphur has been found by Carius to be a mixture of subchloride of sulphur, $S^2 Cl$, with bichloride of sulphur, $S Cl^2$. Niemann * has tried the action of ethylene gas on this body. When the dried gas was passed into the chloride, it was rapidly absorbed with disengagement of heat, and the colour changed from a brown to that of the pure bichloride. When this change of colour was complete, the absorption ceased; for ethylene gas has no action on bichloride of sulphur. The product was then treated with dilute soda ley, by which sulphur was deposited. A large quantity of water was added, and the whole submitted to distillation. A yellowish oil passed over, which was washed with water, and dried over chloride of calcium. It seemed to boil at about

190—200°, but could not be distilled without decomposition, and its purification was therefore very difficult. It is insoluble in water, but is partially soluble in alcohol and in ether. Its alcoholic solution yields a precipitate with chloride of gold, and with protonitrate and pernitrate of mercury.

The most peculiar property of this oil, is that of causing a painful burn when it comes in contact with the hand, which heals with difficulty. The substance analysed was evidently not quite pure; but the results obtained agree best with the formula $C^4 H^4 ClS^2$, which would represent a bisulphide of chlorinated ethyle.

Bussenius and Eisenstück[*] have investigated a rock oil which is obtained from some lias shales near Hanover. The crude oil is distilled with high-pressure steam, and the oil which distils over is treated with sulphuric acid, which removes from it a peculiar bituminous smell. Thus purified it comes into commerce; but the oil for this investigation Bussenius and Eisenstück took as it distils over with the steam, dried it, and submitted it to fractional distillation. It began to boil at 135°, and the temperature gradually rose to 270°. The distillate below 180° was further examined. It was found to be composed mostly of hydrocarbons of the general formula $C_n H_n$, and apparently ranging from $C^{12} H^{12}$ to $C^{18} H^{18}$. Notwithstanding very great labour, it was not found possible to isolate these; nor were the attempts to procure definite compounds from them more successful.

Besides these hydrocarbons, the oil contained a new hydrocarbon, which the authors name *petrol*, which, however, they were not able to separate directly; but when the oil was treated with a mixture of sulphuric and nitric acids, a crystalline nitro-compound of this body was produced: the other hydrocarbons, of which the oil is mostly composed, are not altered even by prolonged contact with this acid mixture. This nitro-compound was purified by repeated crystallizations from alcohol. The analyses of the substance gave for it the formula $C^{16} H^7 (NO^4)^3$, that of the hydrocarbon from which it is derived being $C^{16} H^{10}$. The body was not quite pure, but probably contained some of the nitro-compound of a higher hydrocarbon, $C^{18} H^{12}$.

Trinitropetrol crystallizes in large long needles; it sublimes at 175°. It is not soluble in water, and but slightly so in ether-alcohol, or in benzole; but it dissolves in about 16 parts of boiling alcohol. When it is treated with alcohol it is converted into

[*] Liebig's *Annalen*, February 1860.
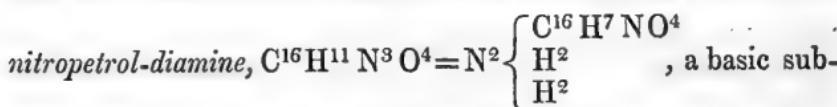
nitropetrol-diamine, $C^{16} H^{11} N^3 O^4 = N^2 \begin{cases} C^{16} H^7 NO^4 \\ H^2 \\ H^2 \end{cases}$, a basic sub-

stance which crystallizes on slowly cooling from an alcoholic solution in large orange-coloured prisms. This body forms crystalline salts with sulphuric and hydrochloric acids. When it is treated with iodide of ethyle, it forms a compound in which three of hydrogen are replaced by ethyle—triethylnitropetrol-

diamine, $N^2 \begin{cases} C^{16} H^7 (NO^4) \\ (C^4 H^5)^2 \\ (C^4 H^5) H. \end{cases}$
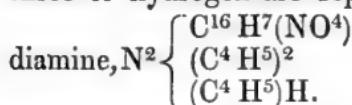
Petrol has the same composition as xylole (Cahours, Church), but, judging from the nature of its derivatives, it does not appear to be identical with it.

Liebig gives a detailed description* of the formation of tartaric acid by the oxidation of milk-sugar by nitric acid. He discusses the mode of occurrence and constitution of tartaric acid, and several allied vegetable acids, and mentions an experiment in which he tried the action of aldehyde on cyanogen dissolved in water in the expectation of effecting the synthesis of malic acid. It gave, however, an unexpected result. A flask containing about two quarts of water was saturated with cyanogen, about an ounce of aldehyde added, and the whole left in a cool place. The fluid remained clear and colourless; but gradually a mass of white crusts separated at the bottom of the flask, which were found to be oxamide. The liquid, saturated for a second and third time with cyanogen, yielded fresh quantities of oxamide. On distilling the liquid some more oxamide separated, and it appeared as if the aldehyde had formed a combination with oxamide which was decomposed by boiling. The aldehyde which distilled over contained some acroleine. The mother-liquor from which the oxamide had deposited contained oxalate of ammonia.

The aldehyde in this experiment, either by its mere presence or by its cooperation, promotes the combination of cyanogen with water, to form, according as it combines with two or four equivalents of water, oxamide or oxalate of ammonia. The aldehyde acts as a sort of ferment; while any other affinity of the cyanogen, for the hydrogen or for the oxygen of the water, appears to be quite suppressed.

* Liebig's *Annalen,* January and February 1860.

LII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 317.]

November 24, 1859.—Major-General Sabine, R.A., Treasurer and
V.P., in the Chair.

THE following communication was read:—

"On Recent Theories and Experiments regarding Ice at or
near its Melting-point." By Professor James Thomson, Queen's
College, Belfast.

My object in the following paper is to discuss briefly the bearings
of some of the leading theories of the plasticity and other properties
of ice at or near its melting-point, on speculations on the same sub-
ject advanced by myself*, and, especially, to offer an explanation of an
experiment made by Professor James D. Forbes, which to him and
others has seemed to militate against the theory proposed by me, but
which, in reality, I believe to be in perfect accordance with that theory.

In the year 1850, Mr. Faraday† invited attention, in a scientific
point of view, to the fact that two pieces of moist ice, when placed in
contact, will unite together, even when the surrounding temperature
is such as to keep them in a thawing state. He attributed this pheno-
menon to a property which he supposed ice to possess, of tending to
solidify water in contact with it, and of tending more strongly to so-
lidify a film or a particle of water when the water has ice in contact
with it on both sides than when it has ice on only one side.

In January 1857, Dr. Tyndall, in a paper (by himself and Mr.
Huxley) read before the Royal Society and in a lecture delivered
at the Royal Institution, adopted this fact as the basis of a theory
by which he proposed to explain the viscosity or plasticity of ice,
or its capability of undergoing change of form, which was pre-
viously known to be the quality in glaciers in virtue of which their
motion down their valleys is produced by gravitation. Designating
Mr. Faraday's fact under the term "regelation," Dr. Tyndall de-
scribed the capability of glacier ice to undergo changes of form, as
being not true viscosity, but as being the result of vast numbers of
successively occurring minute fractures, changes of position of the
fractured parts, and regelations of those parts in their new positions.
The terms *fracture* and *regelation* then came to be the brief expres-
sion of his idea of the plasticity of ice. He appears to have been led
to deny the applicability of the term viscosity through the idea that
the motion occurs by starts due to the sudden fractures *of parts in
themselves not viscous or plastic*. The crackling, he pointed out
might, according to circumstances, be made up of separate starts
distinctly sensible to the ear and to the touch, or might be so slight

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* Proceedings of Royal Society, May 1857. Also British Association Proceed-
ings, Dublin Meeting, 1857. Also Philosophical Magazine, S. 4. vol. xiv. p. 548.
  † Lecture by Mr. Faraday at the Royal Institution, June 7, 1850; and Report
of that Lecture, Athenæum, 1850, p. 640.

and so rapidly repeated as to melt almost into a musical tone. He
referred to slight irregular variations in the bending motion of the
line marked by a row of pins on a glacier by Prof. Forbes, as being
an indication of the absence of any quality that could properly be
called viscosity, and of the occurrence of successive fractures and
sudden motions in a material not truly viscous or plastic. I can only
understand his statements on this subject by supposing that he con-
ceived the material between the cracks to be rigid, or permanent in
form, when existing under strains within the limit of its strength, or
when strained less than to the point of fracture.

  This theory appeared to me to be wrong*; and I then published,
in a paper communicated to the Royal Society, a theory which had
occurred to me mainly in or about the year 1848, or perhaps 1850;
but which, up till the date of the paper referred to, had only been
described to a few friends verbally.   That theory of mine may be
sketched in outline as follows:—If to a mass of ice at its melting-
point, pressures tending to change its form be applied, there will be
a continual succession of pressures applied to particular parts—
liquefaction occurring in those parts through the lowering of the
melting-point by pressure—evolution of the cold by which the so
melted portions had been held in the frozen state—dispersion of the
water so produced in such directions as will afford relief to the
pressure—and recongelation, by the cold previously evolved, of the
water on its being relieved from this pressure: and the cycle of
operations will then begin again; for the parts re-congealed, after
having been melted, must in their turn, through the yielding of other
parts, receive pressures from the applied forces, thereby to be again
liquefied and to proceed through successive operations as before.
  Professor Tyndall, in papers and lectures subsequent to the publi-

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* While the offering of my own theory as a substitute for Professor Tyndall's
views seems the best argument I can adduce against them, still I would point to
one special objection to his theory. No matter how fragile, and no matter how much
fractured a material may be, yet if its separate fractured parts be not possessed of
some property of internal mobility, I cannot see how a succession of fractures is
to be perpetuated. A heap of sand or broken glass will either continue standing,
or will go down with sudden falls or slips, after which a position of repose will be
attained; and I cannot see how the addition of a principle of reunion could tend
to reiterate the fractures after such position of repose has been attained.   When
these ideas are considered in connexion with the fact that while ice is capable of
standing, without immediate fall, on the side of a precipitous crevasse, or of lying
without instantaneous slipping on a steeply sloping part of a valley, it can also
glide along, with its surface nearly level, or very slightly inclined, I think the
improbability of the motion arising from a succession of fractures of a substance
having its separate parts devoid of internal mobility will become very apparent.
If, on the other hand, any quality of internal mobility be allowed in the fragments
between the cracks, a certain degree at least of plasticity or viscosity is assumed,
in order to explain the observed plasticity or viscosity. That fractures—both
large and exceedingly small—both large at rare intervals, and small, momentarily
repeated—do, under various circumstances, arise in the plastic yielding of masses
of ice, is, of course, an undoubted fact: but it is one which I regard not as the
cause, but as a consequence, of the plastic yielding of the mass in the manner
supposed in my own theory. It yields by its plasticity in some parts until other
parts are overstrained and snap asunder, or perhaps also sometimes slide suddenly
past one another.

cation of this theory, appears to adopt it to some extent, and to endeavour to make its principles cooperate with the views he had previously founded on Mr. Faraday's fact of so called " regulation "*.

Professor James D. Forbes adopts Person's view, that the dissolution of ice is a *gradual,* not a *sudden* process, and so far resembles the tardy liquefaction of fatty bodies or of the metals, which in melting pass through intermediate stages of softness or viscosity. He thinks that ice must essentially be colder than water in contact with it; that between the ice and the water there is a film varying in local temperature from side to side, which may be called plastic ice, or viscid water; and that through this film heat must be constantly passing from the water to the ice, and the ice must be wasting away, though the water be what is called *ice-cold.*

There is a manifest difficulty in conceiving the possibility of the state of things here described: and I cannot help thinking that Professor Forbes has been himself in some degree sensible of the difficulty; for in a note of later date by a few months than the paper itself, he amends the expression of his idea by a statement to the effect that if a small quantity of water be enclosed in a cavity in ice, it will undergo a gradual " *regelation* ;" that is, that the ice will in this case be gradually increased instead of wasted. In reference to the first case, I would ask,—What becomes of the cold of the ice, supposing there to be no communication with external objects by which heat might be added to or taken from the water and ice jointly considered? Does it go into the water and produce viscidity beyond the limit of the assumed thin film of viscid water at the surface of the ice? Precisely a corresponding question may be put relatively to the second case—that of the large quantity of ice enclosing a small quantity of water in which the reverse process is assumed to occur. Next, let an intermediate case be considered—that of a medium quantity of water in contact with a medium quantity of ice, and in which no heat, nor cold, practically speaking, is communicated to the water or the ice from surrounding objects. This, it is to be observed, is no mere theoretical case, but a perfectly feasible one. The result, evidently, if the previously described theories be correct, ought to be that the mixture of ice and water ought to pass into the state of uniform viscidity. Prof. Forbes's own words distinctly deny the permanence of the water and ice in contact in their two separate states, for he says, " bodies of different temperatures cannot continue so without interaction. The water *must* give off heat to the ice, but it spends it in an insignificant thaw at the surface, *which therefore wastes even though the water be what is called ice-cold.*" Now the conclusion arrived at, namely, that a quantity of viscid water could be

* I suppose the term regelation has been given by Prof. Tyndall as denoting the second, or mending stage in his theory of "*fracture* and *regelation.*" Congelation would seem to me the more proper word to use after fracture, as *regelation* implies previous melting. If my theory of *melting by pressure and freezing again on relief of pressure* be admitted, then the term regelation will come to be quite suitable for a part of the process of the union of the two pieces of ice, though not for the whole, which then ought to be designated as the process of *melting* and *regelation.*

produced in the manner described, is, I am satisfied, quite contrary to all experience. No person has ever, by any peculiar application of heat to, or withdrawal of heat from, a quantity of water, rendered it visibly and tangibly viscid. We even know that water may be cooled much below the ordinary freezing-point and yet remain fluid.

Professor Forbes regards Mr. Faraday's fact of regelation as being one which receives its proper explanation through his theory described above; and, in confirmation of the supposition that ice has a tendency to solidify a film of water in contact with it, and in opposition to the theory given by me, that the regelation is a consequence of the lowering of the melting-point in parts pressed together, he adduces an experiment made by himself, which I admit presents a strong appearance of proving the influence of the ice in solidifying the water, to be not essentially dependent on pressure. This experiment, however, I propose to discuss and explain in the concluding part of the present paper.

Professor Forbes accepts my theory of the plasticity of ice as being so far correct that it points to *some* of the causes which may reasonably be considered, under peculiar circumstances, to impart to a glacier a portion of its plasticity. In the rapid alternations of pressure which take place in the moulding of ice under the Bramah's press, it cannot, he thinks, be doubted that the opinions of myself and my brother Professor Wm. Thomson are verified [*].

Mr. Faraday, in his recently published 'Researches in Chemistry and Physics,' still adheres to his original mode of accounting for the phenomenon he had observed, and for which he now adopts the name "regelation;" or, at least, while alluding to the views of Prof. Forbes as possibly being admissible as correct, and to the explanation offered by myself as being probably true in principle, and possibly having a correct bearing on the phenomena of regelation, he considers that the principle originally assumed by himself may after all be the sole cause of the effect. The principle he has in view, he then states as being, when more distinctly expressed, the following :—" In all uniform bodies possessing cohesion, *i. e.* being either in the liquid or the solid state, particles which are surrounded by other particles having the like state with themselves tend to preserve that state, even though subject to variations of temperature, either of elevation or depression, which, if the particles were not so surrounded, would cause them instantly to change their condition." Referring to water in illustration, he says that it may be cooled many degrees below 32° Fahr., and still retain its liquid state; yet that if a piece of the same chemical substance—ice—at a higher temperature be introduced, the cold water freezes and becomes warm. He points out that it is certainly not the change of temperature which causes the freezing; for the ice introduced is warmer than the water; and he says he assumes that it is the difference in the condition of cohesion existing on the different sides of the changing particles which sets them free and

* Forbes 'On the Recent Progress and Present Aspect of the Theory of Glaciers,' p. 12 (being Introduction to a volume of Occasional Papers on the Theory of Glaciers), February 1859.

causes the change. Exemplifying, in another direction, the principle he is propounding, he refers to the fact that water may be exalted to the temperature of 270° Fahr., at the ordinary pressure of the atmosphere, and yet remain water; but that the introduction of the smallest particle of air or steam will cause it to explode, and at the same time to fall in temperature. He further alludes to numerous other substances—such as acetic acid, sulphur, phosphorus, alcohol, sulphuric acid, ether, and camphine—which manifest like phenomena at their freezing- or boiling-points, to those referred to as occurring with the substance of water, ice, and steam; and he adverts to the observed fact that the contact of extraneous substances with the particles of a fluid usually sets these particles free to change their state, in consequence, he says, of the cohesion between them and the fluid being imperfect; and he instances that glass will permit water to boil in contact with it at 212° Fahr., or by preparation can be made so that water will remain in contact with it at 270° Fahr. without going off into steam; also that glass can be prepared so that water will remain in contact with it at 22° Fahr. without solidification, but that an ordinary piece of glass will set the water off at once to freeze.

He afterwards comes to a point in his reasoning which he admits may be considered as an assumption. It is "that many particles in a given state exert a greater sum of their peculiar cohesive force upon a given particle of the like substance in another state than few can do; and that as a consequence a water particle with ice on one side, and water on the other, is not so apt to become solid as with ice on both sides; also that a particle of ice at the surface of a mass [of ice] in water is not so apt to remain ice as when, being within the mass there is ice on all sides, temperature remaining the same." This supposition evidently contains two very distinct hypotheses. The former, which has to do with ice and water present together, I certainly do regard as an assumption, unsupported by any of the phenomena which Mr. Faraday has adduced. The other, which has to do with a particle of ice in the middle of continuous ice, and which assumes that it will not so readily change to water, as another particle of ice in contact with water, I think is to be accepted as probably true. I think the general bearing of all the phenomena he has adduced is to show that the particles of a substance when existing all in one state only, and in continuous contact with one another, or in contact only under special circumstances with other substances, experience *a difficulty of making a beginning of their change of state,* whether from liquid to solid, or from liquid to gaseous, or probably also from solid to liquid: but I do not think anything has been adduced showing a like difficulty as to their undergoing a change of state, when the substance is present in the two states already, or when a beginning of the change has already been made. I think that when water and ice are present together, their freedom to change their state on the slightest addition or abstraction of heat, or the slightest change of pressure, is perfect. I therefore cannot admit the validity of Mr. Faraday's mode of accounting for the phenomena of regelation.

Thus the fact of regelation which Prof. Tyndall has taken as the basis of his theory for explaining the plasticity of ice, does in my opinion as much require explanation as does the plasticity of ice which it is applied to explain. The two observed phenomena, namely the tendency of the separate pieces of ice to unite when in contact, and the plasticity of ice, are indeed, as I believe, cognate results of a common cause. They do not explain one another. They both require explanation; and that explanation, I consider, is the same for both, and is given by the theory I have myself offered.

I now proceed to discuss the experiment by Prof. Forbes, already referred to as having been adduced in opposition to my theory. He states that mere *contact* without pressure is sufficient to produce the union of two pieces of moist ice *; and then states, as follows, his experiment by which he supposes that this is proved :—"Two slabs of ice, having their corresponding surfaces ground tolerably flat, were suspended in an inhabited room upon a horizontal glass rod passing through two holes in the plates of ice, so that the plane of the plates was vertical. Contact of the even surfaces was obtained by means of two very weak pieces of watch spring. In an hour and a half the cohesion was so complete, that, when violently broken in pieces, many portions of the plates (which had each a surface of twenty or more square inches) continued united. In fact it appeared as complete as in another experiment where similar surfaces were pressed together by weights." He concludes that the effect of pressure in assisting 'regelation' is principally or solely due to the larger surfaces of contact obtained by the moulding of the surfaces to one another.

I have myself repeated this experiment, and have found the results just described to be fully verified. It was not even necessary to apply the weak pieces of watch-spring, as I found that the pieces of ice, on being merely suspended on the glass rod in contact, would unite themselves strongly in a few hours. Now this fact I explain by the capillary forces of the film of interposed water as follows :—First, the film of water between the two slabs—being held up against gravity by the capillary tension, or contractile force, of its free upper surface, and being distended besides, against the atmospheric pressure, by the same contractile force of its free surface round its whole perimeter, except for a very small space at bottom, from which water trickles away, or is on the point of trickling away—exists under a pressure which, though increasing from above downwards, is everywhere, except at that little space at bottom, less than the atmospheric pressure. Hence the two slabs are urged towards one another by the excess of the external atmospheric pressure above the internal water pressure, and are thus pressed against one another at their places of contact by a force quite notable in its amount. If, for instance, between the two slabs there be a film of water of such size and form as might be represented by a film one inch square, with its upper and lower edges horizontal, and with water trickling from its lower edge, it is easy to show that the slabs will be pressed together by a force

* " On some Properties of Ice near its Melting-Point," by Prof. Forbes, Phil. Mag. 1858, vol. xvi. p. 544.

equal to the weight of half a cubic inch of water. But so small a film as this would form itself even if the two surfaces of the ice were only very imperfectly fitted to one another. If, again, by better fitting, a film be produced of such size and form as may be represented by a square film with its sides 4 inches each, the slabs will be urged together by a force equal to the weight of half a cube of water, of which the side is 4 inches; that is, the weight of 32 cubic inches of water or 1·15 pound, which is a very considerable force. Secondly, the film of water existing, as it does, under less than atmospheric pressure, has its freezing-point raised in virtue of the reduced pressure ; and it would therefore freeze even at the temperature of the surrounding ice, namely the freezing-point for atmospheric pressure. Much more will it freeze in virtue of the cold given out in the melting by pressure of the ice at the points of contact, where, from the first two causes named above, the two slabs are urged against one another.

The freezing of ice to flannel or to a worsted glove on a warm hand is, I consider, to be attributed partly to capillary attraction acting in similar ways to those just described ; but in many of the observed cases of this phenomenon there will also be direct pressures from the hand, or from the weight of the ice, or from other like causes, which will increase the rapidity of the moulding of the ice to the fibres of the wool.

December 8.—Sir Benjamin C. Brodie, President, in the Chair.

The following communication was read :—

"On the Analytical Theory of the Attraction of Solids bounded by Surfaces of a Class including the Ellipsoid." By W. F. Donkin, Esq., M.A., F.R.S. &c.

The surface of which the equation is

$$f(x, y, z, h, k) = 0, \quad \cdots \quad \cdots \quad (1)$$

is called for convenience "the surface (h, k)." The space, or solid, included between the surfaces (h_1, k), (h_2, k), is called "the shell $\left(\begin{smallmatrix} h_2 \\ h_1 \end{smallmatrix}, k\right)$;" and that included between the surfaces (h, k_1), (h, k_2) is called "the shell $\left(h, \begin{smallmatrix} k_2 \\ k_1 \end{smallmatrix}\right)$." [This notation is borrowed, with a slight alteration, from Mr. Cayley.] It is assumed that the equation (1) represents closed surfaces for all values of the parameters h, k, within certain limits, and that (within these limits) the surface (h, k) is not cut by either of the surfaces $(h + dh, k)$, $(h, k + dk)$. It is also supposed that there exists a value h_∞ of h, for which the surface (h_∞, k) extends to infinity in every direction. Lastly, it is supposed that if k be considered a function of x, y, z, h, by virtue of (1), the two following partial differential equations are satisfied :

$$\frac{d^2 k}{dx^2} + \frac{d^2 k}{dy^2} + \frac{d^2 k}{dz^2} = \phi(h),$$

$$\left(\frac{dk}{dx}\right)^2 + \left(\frac{dk}{dy}\right)^2 + \left(\frac{dk}{dz}\right)^2 + n\frac{dk}{dh} = 0 ;$$

in which $\psi(h)$ is any function of h (not involving k), and n is any

constant independent of h and k. The following propositions are then demonstrated :—

The potential, on a given external point, of a homogeneous solid bounded by the surface (h, k), varies as the mass of the solid, if h vary while k remains constant.

The potentials, on a given external point, of the homogeneous shells $\left(h_1, \dfrac{k_2}{k_1}\right)$, $\left(h_2, \dfrac{k_2}{k_1}\right)$ are proportional to the masses of the shells.

The homogeneous shell $\left(h, \dfrac{k_2}{k_1}\right)$ exercises no attraction on an interior mass.

The external equipotential surfaces of the homogeneous infinitesimal shell $\left(h_1, \dfrac{k+dk}{k}\right)$, are the surfaces (h, k), in which h is arbitrary and k invariable[*].

The potential of the homogeneous infinitesimal shell $\left(h_1, \dfrac{k+dk}{k}\right)$ upon an *exterior* point, is

$$\frac{4\pi}{n}\,dk\,\psi\,(h_1)\int_h^{h\infty}\frac{dh}{\psi(h)},$$

and upon an *interior* point, is

$$\frac{4\pi}{n}\,dk\,\psi\,(h_1)\int_{h_1}^{h\infty}\frac{dh}{\psi(h)}.$$

(In these expressions $\psi(h)$ is $\epsilon^{\frac{1}{n}\int\phi(h)dh}$, and h at the lower limit in the first, is the parameter of the surface (h, k) which passes through the attracted point. The density of the shell is supposed to be unity.)

The potential of the finite homogeneous shell $\left(h_1, \dfrac{k''}{k'}\right)$ (density $=1$) upon an exterior point (ξ, η, ζ), is

$$\frac{4\pi}{n}\,\psi(h_1)\left\{\,k''\int_{h''}^{h''\infty}\frac{dh}{\psi(h)}-k'\int_{h'}^{h\infty}\frac{dh}{\psi(h)}+\int_{h'}^{h''}\frac{k\,dh}{\psi(h)}\right\}:$$

in this expression it has been assumed (for simplicity) that h_∞ is independent of k. Also h'', h' are the values of h corresponding to k'', k', when h and k vary subject to the relation $\mathrm{f}\,(\xi, \eta, \zeta, h, k)=0$; and k, in the last integral, is the function of h, ξ, η, ζ determined by this relation.

The differential equations (2) are satisfied in the case of the ellipsoid. For if we put its equation in the form

$$\frac{x^2}{a^2+h}+\frac{y^2}{b^2+h}+\frac{z^2}{c^2+h}=k,$$

it is evident on inspection that

$$\frac{d^2k}{dx^2}+\frac{d^2k}{dy^2}+\frac{d^2k}{dz^2}=2\left(\frac{1}{a^2+h}+\frac{1}{b^2+h}+\frac{1}{c^2+h}\right),$$

and

$$\left(\frac{dk}{dx}\right)^2+\left(\frac{dk}{dy}\right)^2+\left(\frac{dk}{dz}\right)^2+4\frac{dk}{dh}=0.$$

In this case we find $\psi(h)=\left((a^2+h)(b^2+k)(c^2+h)\right)^{\frac{1}{2}}$, and the above general expressions lead to the known results.

[*] It is known that the last two propositions imply the first two (see Mr. Cayley's " Note on the Theory of Attraction," Quarterly Journal of Mathematics, vol. ii. p. 338) ; though this is not the order of proof in the present paper.

February 15, 1860.—Sir C. Lyell, Vice-President, in the Chair.

The following communications were read :—

1. " On the Probable Glacial Origin of some Norwegian Lakes."
By T. Codrington, Esq., F.G.S.

The lakes to which attention was called by this paper are those
frequently found situated at a short distance from the head of the
several fjords on the western coast of Norway. The fjord and the
valley in which such a lake or " vand " lies are parts of one great
chasm, with perpendicular sides, often thousands of feet high. The
valley generally shows traces of the former existence of a glacier,
and is now traversed by a rapid river, which falls into a vand or lake
six or seven miles long, rarely a mile wide, and very deep. The
lake is separated from the fjord by a mass of rolled stones, shingle,
and coarse sand roughly stratified, and sometimes rising 120 feet
above the lake. Through this an outlet has been cut to the fjord,
a distance varying from about one to four miles. On the side
towards the lake this mound is terraced ; and at the upper end of
the lake similar terraces are sometimes seen. The author, with
some doubt, attributes the accumulation of this terraced barrier to
glacial action.

2. " On the Drift and Gravels of the North of Scotland." By
T. F. Jamieson, Esq. Communicated by Sir R. I. Murchison, F.G.S.

In a former communication the author gave an account of some
features of the Pleistocene deposits along the coast of Aberdeenshire,
showing that in certain localities remains of marine animals occur, of
a character similar to those met with in the later Tertiary beds of the
Clyde district, and, like them, indicating the presence of a colder
sea. In the present paper the author treated of the Drift of the higher
grounds in the interior of the country, more especially as regards
that part of Scotland lying between the Moray Firth and the Firth
of Tay. The following phænomena were more particularly de-
scribed :—1. The upper gravels, their distribution and origin ; 2.
the marine drift of the higher grounds and of the highland glens ;
3. the striated and polished rock-surfaces beneath the Drift ; 4.
the high-lying boulders, and the dispersion of blocks from the Ben
Muic-Dhui Mountains. The probability of extensive glacier-action
before the formation of the Drift, the extinction of the land-fauna
preceding the Drift, and the sequence of events during the Pleistocene
period were then dwelt upon ; and the author expressed his opinion
that the following course of events may be supposed to have occurred
in the Pleistocene history of Scotland. 1st. A period when the
country stood as high as, or probably higher than at present, with
an extensive development of glaciers and land-ice, which polished
and striated the subjacent rocks, transported many of the erratic
blocks, destroyed the pre-existing alluvium, and left much boulder-
earth in various places. 2ndly. To this succeeded a period of sub-
mergence, when the sea gradually advanced until almost the whole

country was covered. This was the time of the marine drift with floating ice. The beds with arctic shells belonged to it, and some of the brick-clays are probably but the fine mud of the deeper parts of the same sea-bottom. 3rdly. The land emerged from the water, during which emergence the preceding drift-beds suffered much denudation, giving rise to the extensive superficial accumulations of water-rolled gravel that now overspread much of the surface. This movement continued until the land obtained a higher position than it now has, and became connected with the continent of Europe. Its various islands were probably also more or less in conjunction. The present assemblage of animals and plants gradually migrated hither from adjoining lands. Glaciers may have still been formed in favourable places, but probably never regained their former extension. 4thly. The land sank again until the sea in most places reached a height of from 30 to 40 feet above the present tide-mark. Patches of forest-ground were submerged along the coast. The clays and beds of silt, forming the " carses " of the Forth, Tay, and other rivers, were accumulated, as well as the post-tertiary beds of the Clyde, &c., described by Mr. James Smith, the shells of which agree with those of our present seas. 5thly. An elevation at length took place, by which the land attained its present level. As Mr. Smith has shown, this probably occurred before the Roman invasion : but that man had previously got into the country appears from the fact that the elevated beds of silt near Glasgow, contain overturned and swamped canoes with stone implements.

February 29, 1860.—L. Horner, Esq., President in the Chair.

The following communication was read :—

"On the Lower Lias of the South of England." By Dr. T. Wright, F.G.S.

The author first stated that the uppermost beds of the Lower Lias are those containing *Hippopodium ponderosum*, and that the lowest beds are those with *Ammonites Planorbis*, overlying a series of strata containing *Estheria*, &c., which he separates from the Lias, under the name of the *Avicula contorta* beds. The last rest on the grey and red marls of the Keuper.

Dr. Wright then proceeded with the description of the *A. contorta* beds, including the " Bone-bed," having first enumerated the authors who have written on these and the equivalent strata (Kössener Schichten, &c.) on the Continent. The sections at Garden Cliff, near Westbury on the Severn, at Wainlode Cliff, at Aust Cliff, at Penarth near Cardiff, at Uphill near Weston-super-Mare, at Culverhole near Axmouth, at Wilmcote and Binton near Stratford-on-Avon, were described in detail as illustrating this series ; and General Portlock's section of these beds in the North of Ireland was also alluded to. *Pecten Valoniensis, Cardium Rhæticum,* and *Avicula contorta* are the chief molluscan fossils of this zone.

The next group of strata are those with *Ammonites Planorbis* and *Am. Johnstoni.* Some of the foregoing sections expose these beds, such as those at Uphill and Wilmcote ; but they can be still better studied at Street in Somersetshire, where they have yielded so

many fine Enaliosaurian fossils. These beds are also well exposed at Brockeridge and Defford in the Vale of Gloucester, and at Binton in Warwickshire.

Isastræa Murchisonæ occurs in this zone, and *Ostrea liassica* is very characteristic of some of its lower beds. *Ichthyosauri* and *Plesiosauri* of several species are found in this series; the latter chiefly in the lower part. Of the two known specimens of *Pl. megacephalus*, one was found in these beds near Street, Somerset, and the other at Wilmcote, Warwickshire.

The *Ammonites Bucklandi* characterizes the next higher group of strata, which are also known as the Lima-beds. These are well seen at Lyme Regis, at the Church Cliff and from the Broad Ledge to the shore, and yield several species of *Ichthyosaurus*, also *Am. Conybeari*, *A. rotiformis*, *A. angulatus*, *A. Greenoughii*, and *A. tortilis*.

The *Am. Turneri* beds are next, and can also be studied at Lyme Regis; they have yielded three species of *Ichthyosaurus*. *Am. semicostatus* and *A. Bonnardi* belong to this zone.

The *Am. obtusus* beds succeed, between the Broad Ledge at Lyme and Cornstone Ledge near Charmouth; they apparently have no saurian fossils. *A. Brooki*, *A. stellaris*, *A. planicosta*, and *A. Dudressieri* accompany *A. obtusus*.

The next zone is that of the *Am. oxynotus*, with *A. bifer* and *A. lacunatus*. The beds with *Am. raricostatus* comprise (in ascending order) the Ammonite-bed, the Hippopodium-bed, the coral-band, and the Gryphæa-bed. This zone is well seen near Cheltenham, at Lyme, and at Robin's Hood Bay in Yorkshire. *Am. armatus*, *A. nodulosus*, and *A. Guibalianus* belong to the *A. raricostatus* beds.

Dr. Wright then pointed out that the *Avicula contorta* beds, like the Kössen beds, contain a fauna special to themselves, and might as well be classed with the Trias as with the Lias. They have a wide range in the South of England, South Wales, the Midland Counties, and the North of Ireland. After some remarks on the more important features of the several Ammonite-zones of the Lower Lias, the author concluded by remarking that, as Quenstedt and Oppel had observed, the Middle Lias could be similarly subdivided by means of the Ammonites peculiar to its several stages.

March 14, 1860.—L. Horner, Esq., President, in the Chair.

The following communications were read:—

1. "On the Occurrence of *Lingula Credneri* in the Coal-measures of Durham." By J. W. Kirkby, Esq. Communicated by T. Davidson, Esq., F.G.S.

As the *Lingula Credneri* of Geinitz, formerly known only in the Permian rocks (Lower Permian of Germany; Marlslate of Durham and Northumberland), has of late been found by Mr. Kirkby in the Coal-measures at the Ryhope Winning, near Sunderland, he offers this notice as of interest both as to the discovery of another species common to the faunæ of the Carboniferous and Permian eras, and as illustrative of some of the physical conditions which obtained during

the deposition of the Upper Coal-measures of the North of England, the occasional occurrence of this *Lingula* proving that marine conditions prevailed at intervals in the Durham area during the accumulation of those deposits.

The species now known to be common to the Carboniferous and Permian faunæ (besides *L. Credneri*) are *Terebratula Sacculus*, Mart. (*T. sufflata*, Schl.), *Spirifera Urii*, Flem. (*Martinia Clannyana*, King), *Spiriferina costata*, Schl. (*Sp. octoplicata*, Sow.), *Camarophoria Crumena*, Mart. (*Terebratula Schlotheimii*, v. Buch), *Camarophoria globulina*, Phil. (*Terebratula rhomboidea*, Phil.),—on the authority of Mr. Davidson; *Cythere elongata*, Münst., *C. inornata*, M'Coy, *Bairdia gracilis*, M'Coy,—on the authority of Mr. Rupert Jones; *Gyracanthus formosus*, Ag.,—according to Messrs. King and Howse; *Pinites Brandlingi*, Lindl., *Trigonocarpon Noeggerathi*, Brong., *Sigillaria reniformis*, Brong., *Calamites approximatus*, Brong., and *C. inæqualis* (?), Brong.,—collected by Mr. Howse in the lowest Permian sandstone. From the preceding list of Carboniferous species found also in the Permian strata of Durham, we are able (says the author) to see at a glance the specific relationship (so far as at present known) which exists between the life-groups of the later palæozoic periods. The generic affinity of these groups has long been noticed. This affinity and other apparent indications of a want of systematic difference originated the proposal that the Permian should be included in the Carboniferous system; and Mr. Kirkby considers that the existence of the several recurrent Carboniferous species in the Permian rocks strongly supports this view, and that " Permian " should be retained only as a subordinate term.

2. " On the Rocks, Ores, and other Minerals on the property of the Marquis of Breadalbane in the Highlands of Scotland." By C. H. G. Thost, Esq. Communicated by Prof. J. Nicol, F.G.S.

After noticing generally the mica-schist of the district, with its limestone or calcareous schist, and occasional roofing-slate, the author proceeded to describe first the porphyry-vein (half a mile wide), containing silver-ore, copper-pyrites, grey copper-ore, iron-pyrites, and molybdena, and crossing a vein of non-metalliferous greenstone, at Tomnadashan, on Loch Tay opposite Ben Lawers. He then pointed out the probable connexion of the existing great valleys with lines of fracture due to igneous violence. The veins at Ardtallanaig, containing heavy spar, and ores of zinc, copper, and iron, were next noticed. At Correbuich there are two sets of veins in the calcareous schist; those having a North and South direction contain argentiferous galena and traces of gold. The most eastern hills on Loch Tay, in the neighbourhood of Taymouth abound with quartzose veins containing copper-pyrites, iron-pyrites, and galena. The iron-ore of Glenqueich, and the serpentine and chromate of iron at Corycharmaig, where graphite and rutile also occur, were next noticed. At Lochearn Head there are galena-veins in calcareous schist; here, too, some auriferous arsenical pyrites has been found. Lastly the author described in some detail the lead-bearing veins at Glen Fallich and Tyndrum, which have been worked for many years.

LIII. *Intelligence and Miscellaneous Articles.*

NOTE ON THE SPECIFIC GRAVITY OF ELECTRO-DEPOSITED
AMORPHOUS ANTIMONY*. BY G. GORE, ESQ.

THE following experiments are intended to illustrate the range of variation of specific gravity to which amorphous antimony is liable.

Ten bars, each $1\frac{1}{4}$ inch long, were simultaneously formed, with their ends uppermost, in two rows of five each, upon the two opposite surfaces of a vertical sheet of silver, in a solution composed of teroxide of antimony and hydrochloric acid; and after being removed from the silver, washed and dried, their weights were taken and their specific gravities determined.

No.	Weight.		Sp. gr. at 60° F.
1.	278·15 grs.	....	5·7421
2.	241·24 ,,	....	5·7534
3.	273·30 ,,	....	5·7536
4.	254·52 ,,	....	5·7609
5.	246·185 ,,	....	5·7647
6.	243·66 ,,	....	5·7653
7.	231·94 ,,	....	5·7725
8.	219·56 ,,	....	5·8223
9.	227·42 ,,	....	5·8327
10.	236·215 ,,	....	5·8330

In the bars there were no cavities to which the differences of specific gravity could be ascribed.

Birmingham, March 31, 1860.

ON THE PRODUCTION OF OZONE BY MEANS OF A PLATINUM WIRE
MADE INCANDESCENT BY AN ELECTRIC CURRENT. BY M. LE ROUX.

If a platinum wire, not too large, be made incandescent by an electric current in such a manner that the ascending flow of hot air which has surrounded the wire comes in direct contact with the nostrils, an odour of ozone is perceived. The experiment may be made in the following manner:—A very fine platinum wire ($\frac{1}{10}$th to $\frac{1}{25}$th of a millimetre) 20 centimetres long is taken; it is formed in any shape, and supported in an almost horizontal position in any suitable manner. A glass funnel of 2 or 3 litres is placed over this, so that the air has sufficient access to the wire. As the neck of the funnel is usually too narrow, it is cut so as to leave an aperture 2 or 3 centimetres in diameter, on which is adjusted a glass chimney of a suitable length; the object of which is to cool the gases heated by the wire. The wire is then made incandescent by means of twelve or fifteen Bunsen's cells. The gas issuing from the chimney is found to have the odour of ozone; iodized starch-papers are altered in a few minutes when placed over the chimney. In this case the air passing over the incandescent wire undergoes a peculiar modification by which it acquires the properties of ozone; but whether this is effected by the electricity acting as a source of heat, or by its own proper action, must be reserved for further experiments.— *Comptes Rendus*, April 2, 1860.

* Compare Phil. Mag. S. 4. vol. xvi. p. 452.

OBSERVATIONS ON THE USE OF INSOLUBLE COMPOUNDS
IN VOLTAIC PILES.   BY M. BECQUEREL.

In the decompositions announced in 1837, effected on insoluble
substances placed in contact with the negative pole of a couple or
battery, I was able to reduce large quantities of different metallic
substances, more especially chloride and sulphide of silver, and sul-
phate and phosphate of lead.   These effects are analogous to the
decomposition of fused chloride of silver, which takes place when
this substance is immersed in acidulated water in contact with a
plate of zinc.

Many years afterwards I recurred to the subject, and showed the
advantage to be derived from the use of insoluble substances in the
construction of voltaic couples.   The couples might be composed of
an oxidizable metal (zinc or iron), a single liquid, generally saline
water, and a conductor of tin, surrounded by one of the substances
mentioned—such as silver, lead, or copper minerals, and in particular
sulphate of lead.

One of the most important applications of these effects was the
electro-chemical treatment of silver and lead ores [*], using in this
case the remarkable action produced by an oxidizable metal on
sulphate of lead in presence of saline water.   Since this time I have
frequently used these sulphate of lead batteries in my electro-che-
mical researches.   They were piles with one liquid; the oxidizable
metal was zinc placed in a sailcloth bag, or in a permeable vessel filled
with saturated solution of salt.   The second conductor consisted of
a bar of charcoal, or a plate of copper, lead, or tin, in contact with
brine saturated with sulphate of lead, or holding it in suspension.
The contents of the vessel in which this latter solution was placed
was often 3000 litres.   Six such couples, united as a battery, gave
pretty strong sparks.   The intensity of their action depends on the
depolarization of the negative plate by the sulphate of lead which
is reduced, and by which the disengagement of hydrogen is pre-
vented.   Besides, the liquid contains sulphate of lead in solution as
well as diffused; for it is soluble in about fifty parts of the saline
solution.   The permeable diaphragm serves to prevent the closing
of the circuit and the destruction of the effect of the pile, by the
precipitation of lead on the zinc when solution of salt is employed.

It is to be observed that the electromotive force of the couple is
the difference of the effect produced by the liquid on the zinc and
on the reduced lead, and that hence it is sufficient to have a rod or
plate of tinned iron or of lead, as negative conductor in contact with
the sulphate of lead.   For some years, masses of sulphate of lead,
produced in the manufacture of sulphuric acid at Dieuze, and sold
at a low price, have thus been reduced to the metallic state.   In the
fusion of the lead it is necessary to take suitable precautions, as it
frequently contains a little sulphate.—*Comptes Rendus*, April 2, 1860.

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[*] *Comptes Rendus,* vol. ii. p. 23; and Becquerel's *Traité d'Electricité,*
vol. ii. p. 355 *et seq.*

LIV. *Crystalline Form not necessarily an indication of definite Chemical Composition; or, on the possible Variation of Constitution in a mineral Species independent of the Phænomena of Isomorphism.* By JOSIAH P. COOKE, *Jun., A.A.S., Professor of Chemistry and Mineralogy in Harvard College, United States of America* *.

IN a memoir presented to the American Academy of Arts and Sciences in September 1855 †, I described two new compounds of zinc and antimony, which I named *stibiobizincyle* and *stibiotrizincyle*, on account of their analogy in composition to the metallic radicals of organic chemistry. The symbols of these compounds are Sb Zn² and Sb Zn³; and they are distinguished by the high perfection of their crystalline forms, the last being still further characterized by a most remarkable property of decomposing water quite rapidly at 100° C. I stated in the same memoir that crystals of these two compounds could be obtained, containing proportions of zinc and antimony differing very widely from those required by the law of definite proportions; and I also traced out the relation between the composition of the crystals, and that of the menstruum in which they are formed. It is my object in the present paper to consider the bearing of these facts, already fully described, on the idea of mineral species, and to offer a few suggestions which I hope may be of service in determining the true chemical formulæ of many minerals, and thus in simplifying the science of mineralogy. But in order to render myself intelligible, it will be necessary to recapitulate very briefly the facts in question, referring to the original memoir for the full details.

* Communicated by the Author.

† Transactions of the American Academy of Arts and Sciences, New Series, vol. v. p. 337.

The crystals both of Sb Zn² and Sb Zn³ can be obtained with great readiness. It is only necessary to melt together the two metals in the atomic proportions, and when the metals are fully alloyed, to proceed exactly as in crystallizing sulphur. The melted mass is allowed to cool until a crust forms on the surface, which is then broken, and the liquid metal remaining in the interior poured out. On subsequently breaking the crucible, the interior is found lined with magnificent metallic crystals, which, when not tarnished by oxidation, have a silver-white lustre. In the course of my investigations on these compounds, crystallizations were made, or attempted, of alloys differing in composition by one half to five per cent., according to circumstances, from the alloy containing 95 per cent. of zinc, to that containing 95 per cent. of antimony; *but only two crystalline forms were observed,* that of Sb Zn² and that of Sb Zn³. The crystals of the two compounds both belong to the trimetric system; but they differ from each other, not only in their crystallographic elements, but also in their whole "habitus." Stibiotrizincyle crystallizes in long acicular prisms, which group themselves together into larger prismatic aggregates; while stibiobizincyle crystallizes in broad plates, which twin together on an octahedral face, and form a very characteristic cellular structure. This very striking difference in the character of the crystals proved to be an important circumstance in the investigation, as it enabled me to distinguish with certainty between the two compounds, even when the faces of the crystals were so imperfect that a measurement of angles was impossible.

The most remarkable result of the investigation, and the one to which I wish to direct especial attention, is the fact that each of the two crystalline forms was found to be constant under very wide variations in the per-centage composition of the crystals. As this is a point of great importance, it will be necessary to enter more into detail, considering in the first place the crystals of Sb Zn³. The crystals of this compound are obtained in the greatest perfection from an alloy containing the two metals in just the proportions represented by the formula, namely, 42·8 parts of zinc, and 57·2 parts of antimony. They are then comparatively large, generally aggregated, and, as the three analyses cited in the accompanying Table indicate, they have the same composition as the alloy.

Composition of the alloy by synthesis.		Composition of the crystals by analysis.		
Per cent. of Zn.	Per cent. of Sb.	Per cent. of Zn	Per cent. of Sb.	Sum.
42·80	57·20	43·15	56·93	100·08
,,	,,	43·06	56·50	99·56
,,	,,	42·83	57·24	100·07

On increasing gradually the amount of zinc in the alloy up to 48·7, the crystals continued to have the composition of the alloy; and the only difference which could be observed in their character was that they were smaller, and more frequently isolated. Between these limits the whole mass of the alloy exhibited a strong tendency to crystallization; and by pouring it, as it cooled, from one vessel to another, it could be crystallized to the last drop. On increasing the amount of zinc in the alloy to 50·7 per cent., the amount of zinc found in the crystals was uniformly less than it was in the alloy; but no closer relation between the two could be detected, owing, undoubtedly, to the unavoidable irregularity in the crystallization of the alloys which contained more than 50 per cent. of zinc. This arose from a peculiar pasty condition which the liquid mass assumed at the point of crystallization. Definite crystals, however, were obtained from an alloy of 60 per cent. zinc containing 55 per cent.; above this the crystals became less and less abundant, and gradually faded out, although the alloy of 86 per cent. of zinc exhibited a radiated crystalline texture; and a trace of this structure could still be discovered even in the alloy containing only 4 per cent. of antimony. It was very interesting to trace the gradual fading out of the crystalline structure, as the character of the phænomenon was entirely analogous to that which may be noticed in many crystalline rocks.

Finding that the crystalline form of Sb Zn^3 was constant under so great an increase of the proportion of zinc in the crystals, it might be supposed that, on returning to the alloy of 42·8 per cent. of zinc and increasing the amount of antimony, we should obtain crystals containing an excess of antimony; but so far is this from being true, that the slightest excess of antimony entirely changes the character of the crystallization. On crystallizing an alloy containing 41·8 per cent. of zinc, not a trace of any prismatic crystals could be seen; but in their place there was found a confused mass of thin metallic scales, which, as will soon be shown, are imperfect crystals of Sb Zn^2. Thus it appears that, although perfectly formed crystals of Sb Zn^3 can be obtained containing 55 per cent. of zinc (that is, 12 per cent. above the typical proportions), they cannot be made to take up the slightest excess of antimony.

Let us pass now to the crystals of Sb Zn^2. In order to obtain crystals having the exact typical constitution, it was found necessary to crystallize an alloy containing not more than 31·5 per cent. of zinc. At this point large compound crystals are obtained corresponding to the large crystals of Sb Zn^3; and the same was true of alloys down to 27 per cent. of zinc. Between these two limits (namely, alloys of 31·5 and 27 per cent. of zinc) the cry-

stals formed were found to have the theoretical composition of
Sb Zn², indicating of course a tendency towards this point; but
on increasing or diminishing the amount of zinc in the alloy
beyond these limits, the composition of the crystals immediately
began to vary in the same direction as that of the alloy.   The
crystals of Sb Zn² containing an excess of zinc are smaller and
more frequently isolated than those having the exact theoretical
composition.   A similar fact, it will be remembered, is true of
the crystals of Sb Zn³.

At the alloy of 33 per cent. of zinc, the definite crystals of
Sb Zn² begin to disappear, and are succeeded by thin metallic
scales, which are obviously imperfect crystals of the same form.
This was established, not only by the obvious law of continuity
noticed in the different specimens (the perfect crystals gradually
passing into the scales), but also by the peculiar mode of twin-
ing, which was the same with the scales as with the large cry-
stals, forming the peculiar cellular structure already referred to.
Moreover, the angle between two scales thus united was found
to be equal to the basal angle of the perfect crystals, at least as
nearly as could be measured.   These scales continue up to the
alloy of 41·8 per cent. of zinc, becoming, however, less abundant
and less distinct.   Several specimens of them were analysed; but
no regularity could be detected in their composition, except that
they all contained a much larger amount of zinc than the alloys
in which they were formed.

Crystals of Sb Zn² containing an excess of antimony were readily
obtained from alloys containing less than 27 per cent. of zinc.
They became more and more imperfect as the excess of antimony
increased, and finally faded out altogether in the alloys below
20 per cent. of zinc.   It is evident, therefore, that definite and
perfect crystals of Sb Zn² can be obtained with a large excess
either of zinc or antimony above the theoretical composition.   It
is also evident that, of the two compounds, Sb Zn² is the most
stable,—first, because it is formed to the exclusion of Sb Zn³ in
all alloys containing less zinc than the amount corresponding to
the typical composition of the last compound; and secondly,
because the crystals retain the typical composition under quite a
wide variation (viz. between 31·5 and 27 per cent.) in the com-
position of the alloy.

The facts above stated are fully illustrated by the following
Table, which gives the results of a large number of analyses of
crystals of both compounds formed in alloys containing different
proportions of the two metals:—

Analyses of the Crystals formed in the Alloys of Zinc and Antimony.

Stibiotrizincyle.					Stibiobizincyle.				
Composition of the alloys by synthesis.		Composition of the crystals by analysis.			Composition of the alloys by synthesis.		Composition of the crystals by analysis.		
Per cent. of Zn.	Per cent. of Sb.	Per cent. of Zn.	Per cent. of Sb.	Sum.	Per cent. of Zn.	Per cent. of Sb.	Per cent. of Zn.	Per cent. of Sb.	Sum.
70·40	29·60	64·15	35·77	99·92	33·00	67·00	35·37	64·57	99·94
66·50	33·50	61·00	39·00	*100·00	33·00	67·00	35·40	64·60	†100·00
64·50	35·50	53·50	41·44	99·94	32·50	67·50	34·62	64·92	99·54
......	......	55·49	44·42	99·91	32·50	67·50	34·61	65·39	†100·00
60·60	39·40	55·00	45·09	100·09	31·50	68·50	33·95	66·09	100·04
58·60	41·40	50·39	49·29	99·68	29·50	70·50	33·62	66·38	†100·00
56·60	43·40	49·92	50·05	99·97	29·50	70·50	33·62	66·38	†100·00
54·70	45·30	48·26	51·42	99·68	27·50	72·50	33·85	65·81	99·66
52·70	47·30	47·47	52·53	†100·00	26·50	73·50	32·08	67·60	99·68
......	......	......	......	......	26·00	74·00	30·74	69·06	99·80
50·70	49·30	46·89	53·11	†100·00	25·50	74·50	30·43	69·51	99·94
50·70	49·30	46·45	53·55	†100·00	25·00	75·00	29·88	70·20	100·08
48·70	51·30	48·66	51·34	†100·00	24·50	75·50	28·76	71·24	100·00
46·70	53·30	46·77	53·23	†100 00	23·50	76·50	27·93	71·85	99·78
44·80	55·20	44·26	55·73	†100·00	22·50	77·50	26·62	73·27	99·89
43·80	56·20	44·01	55·96	†100·00	21·50	78·50	24·83	74·74	99·57
42·80	57·20	43·15	56·93	100·08	20·12	79·88	20·58	79·42	100·00
42·80	57·20	43·06	56·50	99·56					
42·80	57·20	42·83	57·24	100·07					

* In this analysis the antimony only was determined.

† In this analysis the zinc only was determined.

The relation between the composition of the crystals Sb Zn² and that of the alloy in which they are formed, is discussed at length in the memoir already referred to. It is there shown to be a very simple function of the mass of metal which is in excess in the alloy, and of the force which determines the union of the elements in definite proportions. The whole order of these phænomena seem to the author to point to the existence of a power in the mass of metal which is in excess in the alloy, to disturb the action of the force, whatever it may be, which tends to unite the elements in definite proportions. There is, in the first place, a strong tendency in the elements to unite and form crystals having the exact typical composition; and secondly, this tendency is only overcome by a certain excess of either metal in the alloy. Then, again, the crystals of one compound obviously interfere with those of the other. This certainly has the appearance of one force interfering with the action of another, —the force of mass (if I may so call it) perturbing the action of the chemical force. But it is not my object at present to enter into a discussion on the cause of this variation. Moreover, since

such a discussion must be based on purely hypothetical grounds, we could not expect to arrive at any definite conclusion. The facts will be viewed differently according to the theory which may be adopted in regard to that long-controverted subject, the essential constitution of matter. Leaving, however, all theoretical considerations aside, there are certain practical bearings of the *observed facts* on the science of mineralogy which are of immediate application.

Here are two beautifully crystallized products, as well crystallized as any that occur in nature, and yet the different specimens of the crystals differ from each other so widely in composition that any single analysis might lead to an entirely erroneous conclusion in regard to the general formula of the substance. Were a chemist to analyse accidentally solely the crystals obtained from an alloy containing 58·6 per cent. of zinc, he would at once determine that the formula of the compound was $Sb\,Zn^4$; and by a like accident he might be led to any other formula between this and $Sb\,Zn^3$: in fact, by an analysis of a number of specimens of needle-shaped crystals obtained from alloys of copper and tin, Rieffel was led to several just such improbable formulæ; and in my own investigations it was not until I had analysed a whole series of crystals, that the real nature of the phænomena became apparent, and the true constitution of the compounds determined. If, then, such great variations in composition are compatible with a definite crystalline form in these furnace products, may not similar variations occur in the crystalline minerals formed in nature?

It is not necessary to make an extended investigation in order to answer this question; for the materials at our hands are sufficient to give us a satisfactory reply.

There is a compound of antimony and silver called Discrasite, which occurs in many localities crystallized in trimetric prisms homœomorphous with $Sb\,Zn^3$. The formula of the mineral is therefore probably $Sb\,Ag^3$, which would require 71·5 per cent. of silver; but the per cent. as given by analysis varies between 75·25 and 78 per cent., and one analysis gives the per cent. as high as 85. Further analyses of this mineral are required in order to determine its constitution, but there can be no doubt that it varies in composition like $Sb\,Zn^3$.

Silver-glance is another highly crystalline mineral. Theoretically it should contain 87·1 per cent. of silver and 12·9 per cent. of sulphur; but in a specimen analysed by Klaproth, the proportions were 85 and 15.

Again, the analyses of pyrrhotine (magnetic pyrites) give results varying between 38·78 per cent. sulphur, 60·52 per cent. iron (variety from Bodenmais), and 43·63 sulphur, 56·37 iron

(variety from Barèges). The constitution of the mineral is still uncertain; but its true formula is probably Fe S, which would require 36·4 per cent. sulphur and 63·6 per cent. iron. Lastly, the analyses of antimony-glance give results varying between

$$\begin{array}{ccc} \text{Antimony } 74\cdot06, & \text{and} & \text{Antimony } 73\cdot5, \\ \text{Sulphur } \quad 25\cdot94, & & \text{Sulphur } \quad 26\cdot5. \end{array}$$

The true formula of this mineral is undoubtedly $Sb\,S^3$, which would require only 72·88 per cent. of antimony.

Similar examples might be very greatly multiplied. Those just cited were selected at random from the first few pages of Dana's ' System of Mineralogy.' They are all examples of binary compounds which occur almost chemically pure in nature; so that the phænomena in question are not complicated by those of isomorphism.

When we pass to minerals of more complex constitution, the same phænomena can be made evident, although not quite so easily, on account of the introduction of the phænomena of substitution by isomorphous elements. It will not, however, be necessary for me to cite examples; for it is a fact perfectly well known to all mineralogists, that, after making allowances for the substitution of isomorphous elements, the various analyses of such minerals as mica, hornblende, garnet, and tourmaline differ very greatly from each other,—a difference, moreover, which no mere error of analysis will explain, and which must therefore be referred to an actual variation in composition. In the silicates this variation in composition is made evident by the variation of what is termed the "oxygen ratios;" and it is well known to mineralogists that in many species this variation is very large. For example, in mica the following ratios between the oxygen in the base and acid have been observed in merely the Muscovite variety:—$13 : 16$, $13\frac{1}{3} : 16$, and $14\frac{2}{3} : 16$; and similarly wide variations might be pointed out in other well-known species. It is in consequence of such variations as these that the general chemical formulæ of some of the best-known mineral species, such as mica and tourmaline, are still uncertain; and in other cases, where the true formula is probably known, the constitution of the mineral has been determined quite as much from other considerations as from the chemical analyses.

Sufficient has been said, I think, to show that variations in composition similar to those which I have observed in zinc and antimony occur in many minerals; and I trust that the results of my investigation will serve to throw light on this whole class of phænomena, which have so greatly perplexed mineralogists, and rendered all strictly chemical classifications of mineral species so unsatisfactory. This investigation has shown that a defi-

nite crystalline form is compatible with quite a wide variation of composition, and has in this way pointed out an explanation of the variation observed in the mineral kingdom. But more than this, the investigation has also indicated a method by which, amidst all this variation, the true constitution of the mineral can be determined.

In the compounds of zinc and antimony, although the definite crystalline form was compatible with a wide variation in the proportions of the constituent elements, yet the point corresponding to the typical composition was marked by several unmistakeable properties, which clearly enough indicated the true formulæ of the compounds. These properties are discussed at length in my original memoir, and need therefore only to be alluded to in this connexion.

It has already been stated that the crystals, both of $Sb\,Zn^3$ and $Sb\,Zn^2$, having the theoretical composition are, as a rule, larger and more generally aggregated than those containing an excess of either metal. Moreover, in $Sb\,Zn^2$ the general character of the crystals appears to be modified by the change of composition, although the crystallographic elements remain the same. Thus in the crystals having the theoretical composition, the octahedral planes are greatly developed, giving to the crystals the general appearance of a truncated octahedron*. But as the crystals take up an excess either of antimony or zinc, the basal planes become more and more dominant, and the crystals are at last reduced to thin plates. In fact, so marked are these changes, that, after a little experience, a person could tell the approximate composition of the crystals from their general appearance. Similar changes in the appearance of many minerals are familiar to the mineralogist. They are seen in calcite, heavy spar, Anglesite, and others, and may serve as guides in tracing variations of composition.

Again, the specific gravity of the crystals, both of $Sb\,Zn^2$ and $Sb\,Zn^3$, was taken with great care through the whole series, and the results are tabulated below. The union of the two elements is attended with an increase of volume, and this increase is at a maximum at the points corresponding to the theoretical composition. These points would therefore be marked in a set of crystals by being points of minimum specific gravity; and they could be determined with great accuracy by means of this property, even in a series of alloys of the two metals which had not been crystallized. This fact is illustrated by the following Table, reprinted from the original memoir.

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* See figure accompanying my original memoir.

Specific Gravities of Crystals formed in the Alloys of Zinc and Antimony.

Composition of the alloys.		Composition of the crystals.		Spec. grav. of crystals by experiment.	Mean spec. grav. of zinc and antimony.	Expansion in crystallizing.
Per cent. of Zn.	Per cent of Sb.	Per cent. of Zn.	Per cent. of Sb.			
100·00	......	......	......	7·153	7·153	0·000
*96·00	4·00	......	......	7·069	7·133	0·064
*86·20	13·80	......	......	6·898	7·082	0·184
*76·30	23·70	......	......	6·769	7·032	0·263
70·40	29·60	64·20	35·80	6·699	6·975	0·276
66·50	33·50	61·00	39·00	6·628	6·959	0·331
64·50	35·50	58·56	41·44	6·596	6·948	0·352
62·50	37·50	55·53	44·47	6·506	6·933	0·427
60·60	39·40	55·00	45·00	6·440	6·931	0·491
58·60	41·40	50·39	49·61	6·396	6·909	0·513
56·60	43·40	49·95	50·05	6·388	6·906	0·518
48·70	51·30	48·66	51·34	6·404	6·900	0·496
46·70	53·30	46·77	53·23	6·376	6·891	0·515
44·80	55·20	44·26	55·74	6·341	6·879	0·538
†42·80	57·20	43·09	56·91	6·327	6·874	0·547
*40·00	60·00	......	......	6·386	6·860	0·474
*35·00	65·00	......	......	6·404	6·837	0·433
33·00	67·00	35·37	64·63	6·401	6·838	0·437
‡29·50	70·50	33·62	66·38	6·384	6·830	0·446
‡27·50	72·50	33·85	66·15	6·383	6·831	0·448
26·50	73·50	32·08	67·92	6·400	6·822	0·422
26·00	74·00	31·07	68·93	6·418	6·818	0·400
25·50	74·50	30·43	69·57	6·428	6·816	0·388
24·50	75·50	28·76	71·24	6·449	6·807	0·358
22·50	77·50	26·62	73·38	6·453	6·798	0·345
21·50	78·50	24·83	75·17	6·467	6·790	0·323
*15·00	85·00	......	......	6·564	6·744	0·180
*10·00	90·00	......	......	6·603	6·721	0·118
*5·00	95·00	......	......	6·655	6·698	0·043
......	100·00	......	......	6·677	6·677	0·000

* Alloys not crystallized.
† Point of typical composition of Sb Zn^3.
‡ Point of typical composition of Sb Zn^2.

The point of typical composition in the case of the crystals of Sb Zn^3 was still further marked in a most decided manner by a very remarkable property. It has already been stated that this compound has the power of decomposing water with rapidity at 160° C.; but this is true only of those crystals which have approximately the theoretical composition. During the course of my investigation I determined the quantity of hydrogen evolved by alloys of different composition during a given time, taking care, of course, that the circumstances should be the same in all cases; and I found that with the alloy containing 43 per cent. of zinc, there is an immense maximum, confined at most between 2 per cent. on either side, the alloy of 43 per cent. yielding over

nine times as much gas as an alloy of 50 per cent., although the crystals of the last were fully as definite as those of the first*.

It is evident from the above facts, that the points corresponding to the theoretical composition of the two compounds of zinc and antimony, are also *points of maxima and minima* of various properties. Now I have no doubt that the same truth will be found to hold in the mineral kingdom. In a mineral like tourmaline or mica, for example, the specimen having the exact theoretical composition may probably be discovered by examining a large number of specimens, and discussing their various physical properties. All the physical properties may be of value in this connexion, such as lustre, hardness, specific gravity, specific heat, &c.; and no mechanical rules can be laid down. Much must depend on the discretion of the observer; and in any cases such properties will be selected as are best adapted to the circumstances of the case. In comparing different crystals of the same mineral, it is obviously important to select such as have been formed in a different matrix; for it is only with such that we should be led to expect great variations of composition. It is also evident that the phænomena would be complicated when there has been a substitution of isomorphous elements; and until the effect of such substitution on the physical properties can be traced, it will be necessary to select specimens of as uniform a constitution as possible.

With one other consideration I will close this paper. The principle which has been here discussed must modify materially our notion of a mineral species. The idea of a mineral species has hitherto involved chiefly two distinct characters:—first, a definite crystalline form; second, a constant general formula; and any important variation in either of these characters has been regarded as equivalent to a change of species. Rutile and anatase are regarded as different species, because their crystalline forms are slightly different, although both minerals have identically the same constitution; and again, magnetite and Franklinite, which have the same form, are regarded as different species, because they have a slightly different composition. It is true that the actual composition of a mineral may vary very greatly by the substitution of isomorphous elements, and yet, if the general formula remains constant, the species may not be changed. But the extent to which such substitution can be carried without changing the species is not so well settled among mineralogists as could be desired, and the same rule is not applied to all species. The difference between the *varieties* of garnet, for example, is as great as that between the *species* magnetite and Franklinite. Leaving, however, this point undetermined, all mineralogists

* See Table in the memoir before cited.

have agreed that any *essential change* in the *general formula* was inconsistent with the idea of the *same species.* The result, however, of my investigation is to show that the general formula of a mineral species may vary also, or, as I should rather say, the *general formula* is not necessarily the actual formula of each given specimen, but only the *typical formula* of the species towards which the mineral tends, and which it would unquestionably reach if it could be several times recrystallized.

According to this view, the general formula represents not the *actual constitution* of the mineral, but only a certain *typical composition,* which perhaps is never realized with any actual specimen. The fact that the composition of a mineral species may be modified by the substitution of isomorphous elements, was first established by Mitscherlich, and has long been an admitted principle in mineralogy. We must now, as I think, still further expand our idea of a mineral species, and admit that its composition may be modified by an actual variation in the proportions of its constituents. Thus it is that in mineralogy, as in other sciences, we are led to admit the truth of that maxim which every advance in true knowledge seems to verify, " Natura non facit saltus."

While the results of my investigations thus serve to render the idea of a mineral species less definite than before, I cannot but hope that they will tend ultimately to simplify the whole subject of mineralogy ; for not only may we expect to reduce the number of mineral species, but also, by simplifying the general formulæ of those which remain, to classify the whole with a greater precision than is now possible. To do this, however, implies a careful revision of the whole subject-matter of mineralogy on the principles above given,—a labour of which few can appreciate the extent, except those who are familiar with the methods of physical research. The work cannot be done by any one person; and it is the chief object of the present paper to call the attention of mineralogists to the importance of the subject.

I have not thought it necessary to dwell in this paper on the obvious distinction between the phænomena here in consideration, and those of isomorphism. It was shown in my previous memoir, that the variation in the composition of the crystals of $Sb\ Zn^3$ and $Sb\ Zn^2$ could not be explained by this principle ; and the distinction between the two classes of phænomena has been still further illustrated by a recent investigation on the crystals formed in alloys of copper and zinc, made in my laboratory by Mr. F. H. Storer. These crystals, which are undoubtedly mixtures of isomorphous elements, give no indications whatever of points of typical composition,—thus illustrating not only the characters of an isomorphous mixture, but also the distinction

between such a mixture and a true chemical compound. Admitting, then, the possibility of a variation of composition in a mineral species, independent of the phænomena of isomorphism, it becomes of importance to distinguish this new class of phænomena by a separate term; and I would propose for this purpose the word *Allomerism*. By this word I would designate *a variation in the proportions of the constituents of a crystallized compound without any essential change in the crystalline form.* If, then, we also use the word *typical* to indicate the condition of *definite composition*, we may speak of those specimens of a mineral species which contain an excess of one or the other constituent, as *allomeric variations from the typical composition.* The degree of allomerism would then be measured by the excess of the *allomeric constituent* above the typical composition. Thus the crystals of Sb Zn^3 containing 42·3 per cent. of zinc would be said to have the typical composition; while those containing 55 per cent. of zinc would be distinguished as an allomeric variety, the degree of allomerism in this instance amounting to 12 per cent., and zinc being the allomeric constituent. In the case of the mineral Discrasite, it is probable that no specimen having the typical composition has yet been analysed. Those specimens whose analyses are given in Dana's 'System of Mineralogy,' are all probably *allomeric varieties* of the mineral, silver being the allomeric constituent, and the degree of allomerism varying from 4 to 7 per cent. It is unnecessary, however, to multiply examples, as the above are sufficient to illustrate the use of the term.

LV. *On Phænomena observed during Total Eclipses of the Sun. By* Dr. LAMONT, *Astronomer Royal at Munich*[*].

[With a Plate.]

IN my yearly report for 1854, I have endeavoured to give a novel explanation, and one that differed from all the views hitherto entertained, of the violet prominences that are noticed in total solar eclipses. According to the explanation there set forth only in general terms, the phænomenon is produced by little masses of clouds which become condensed in our atmosphere in the cone of the moon's shadow, owing to the depression of the temperature which takes place there. From the circumstance of there occurring in 1860 in Spain a total eclipse of the sun which may be observed under very favourable conditions, and the opportunity there will then be afforded for arriving at data for confirming or refuting my hypothesis, I am induced to return

to the subject, and to discuss more fully the details contained in recent reports upon this matter, without, however, touching upon the fundamental principles set forth at page 8 of my Yearly Report for 1854, with which I assume the reader to be familiar.

At the outset I must remark that when in a dark room an opening in a window-shutter, $a' b'$ (Plate I. fig. 1), is observed from c through a film of vapour A B, the visibility of the vapour depends on the brilliancy of the entering light*. If the brilliancy is *very considerable*, the light from $a b$ falls on the whole breadth of $d e$, and is reflected to the eye from all points of the vapour, thus rendering the entire film of vapour visible, though with very unequal intensity. The intensity will diminish from f to d and from g to e, because the angle of incidence and reflexion is more and more inclined; the greatest intensity will be manifested in the space $f g$, where the light passes from the opening direct through to the eye. The more the brilliancy of the light that enters at the opening in the shutter diminishes, the more must the visibility of the vapour decrease, beginning from d and e; and a limit is soon reached where the side portions $f g$ and $d e$ disappear altogether, the part $f g$ alone remaining visible. We see from this, that when a surface of light of very low intensity is observed through a mass of vapour, *only that portion of the vapour can be perceived which is between the eye and the luminous surface.*

If the opening $a b$ is so disposed that the light beginning at a goes on decreasing towards b, then in like manner the visibility of the vapour will diminish from f towards g, and will only be extended over a certain portion of the space $f g$.

To render what has been stated applicable to the phænomenon of total solar eclipses, it is requisite, in the first place, to form a precise notion with regard to vapour. On examining carefully the formation of clouds, especially as they frequently present themselves to us in summer, it will be remarked that the vapours which congregate together form no regular covering, but a ragged interwoven mass (fig. 2), consisting of thicker parts, and parts growing less dense till they vanish altogether. It will further be noticed that very frequently the vapour disappears at one side or at a corner—melts away as it were—and is deposited on another side.

As far, moreover, as the light is concerned which serves for rendering objects visible during total solar eclipses, it consists of

* If an uncoated looking-glass is besprinkled with alum-water and then allowed to dry, a fine coating is formed thereon, on looking through which the same appearances are presented as through a mass of vapour. This arrangement is suitable to the experiment in question.

a bright ring encircling the edge of the moon, and which at the edge itself is tolerably intense, but the intensity of which disappears very rapidly outwards. It is moreover to be remarked that the light may render the masses of vapour visible in different manners. With a certain density of the vapour, and with a certain intensity (always tolerably subdued) of the source of light, the transmitted light appears *coloured reddish*; with a greater density of the vapour, the light appears *white*; and when the vapour has attained a cloud-like density, the light becomes entirely stopped, the mass of vapour appearing *black*.

Let us now take into consideration, laying aside entirely at first all that relates to motion, what phænomena a mass of vapour between the observer and the ring of light attending a total solar eclipse will produce.

First, let a portion of the moon's edge be covered with a mass of vapour (fig. 3), *a b c d*; in that case the portion situated within the disc of the moon disappears for the observer, as does also that portion extending out beyond the ring of light, and there remains nothing but an elevation between *a* and *b*, the height of which depends on the width of the luminous ring, while its colour is dependent on the circumstances stated above.

Suppose we have (fig. 4) a mass of vapour, *a b*, a corner of which juts out beyond the edge of the moon, we shall then have a mountain-shaped prominence produced, as the figure shows.

If we have (fig. 5) a mass of vapour, *a*, touching the edge of the moon with one corner, there will be seen at the moon's edge a projection with small isolated spots.

Let us now examine also the *motion*. The motion is here threefold.

1. A motion of the earth on its axis, whereby the whole atmosphere, and consequently the masses of vapour suspended therein, are carried along over the moon's disc from west to east.

2. A movement of the clouds in the atmosphere, brought about by currents of air, whose direction, speaking in a general way, is not subject to any rule.

3. A motion due to the vapour becoming dissolved or deposited.

The first of these three motions is alone regular; and its consequence would be that the masses of vapour would apparently become covered up by the western limb of the moon, and would, on the contrary, make their appearance again on the eastern limb.

With respect to the second motion, only thus much can be said,—namely, that it follows the course of the current of the air. When, therefore, the mass of vapour takes its origin in the lower current of air, its motion coincides with the direction of the wind;

but when the vapour forms in the upper current, its motion will probably be contrary to that of the lower one; this latter point, however, is very uncertain.

With regard to the third motion, nothing can be laid down theoretically respecting it. In a general way the consequence is that the apparent motion, considered as forming a part of the three mentioned above, will, both in amount and also in direction, be *accidental.* Nor are we able to lay down anything more definite relative to the changes of form originating in the motion. If in a cloud of uniform density (fig. 3) the motion takes place in the direction *c d*, the elevation on the moon's limb will remain tolerably steady, with the exception of its extension, which alters with the breadth of the cloud. A projecting point (fig. 4) apparently disappears behind the moon's limb if the motion is from *a* towards *b*; a projection becomes converted into a mountain-crest when the mass of vapour approaches towards the moon's centre.

Some kind of motion, however, is always to be looked for; for it would be a most improbable case that all the three motions mentioned above should just counteract each other.

If we compare the theoretical views hitherto promulgated with observation, it will be seen that, generally speaking, observers have not directed their special attention to the circumstances that were essential to arriving at a decision; nevertheless many particulars in confirmation of my explanation may be derived from the great number of reports and sketches that we have before us. The sketch by Fearnley of the solar eclipse of the 28th of July, 1851, as observed by him at Rixhöft, seems to me to be especially characteristic. See fig. 6 for a copy thereof. The form proper to clouds is here so distinctly pronounced, and extends so far away from the moon's edge, that it seems to me impossible to look for the origin of the phænomenon anywhere but in our atmosphere.

I select the following circumstances from the very instructive report of Schmid, who observed the same eclipse at Rastenberg.

In the first seconds of the totality, he at once perceived in the corona "*several brighter places of imperfectly defined form, something like separate, white, very nebulously-blurred little clouds.*" When the sun broke forth in the form of a fine crescent of light, he examined carefully the remaining part of the moon's edge, which disappeared altogether after 1 minute and 55 seconds, after being 40 seconds previously partially interrupted, so that there yet remained only separate portions of the arc which represented the curvature of the moon's edge.

Schmid did not remark any protuberances at all during the

first 7 to 10 seconds; it was not till after that interval that a red protuberance declared itself of a sudden on the north-east, *precisely at the place where, before, a nebulous white spot had been seen*. This protuberance did not proceed from within outwards from the moon's limb, but was formed suddenly, as if "the red of the protuberance had had a whitish nebulous covering removed from off it." It was distinctly remarked how the disc of the moon advanced over this protuberance and completely covered it over in a few seconds. A second protuberance became visible shortly after the first one, but somewhat more to the north, and it disappeared simultaneously with the first one. A third protuberance towards the south-east remained visible only during two seconds. On the western limb there appeared gradually three larger and one very small protuberance, which all increased in size by degrees, and so changed their shape that there is hardly any way of explaining it but on the assumption that a ragged mass of vapour traversed the moon's disc in a westerly direction. The wind on the surface of the earth had a general westerly course, but varied both in its direction and force. We must therefore assume that the masses of vapour were formed in the upper regions of the air, and were borne onward in a direction opposed to that of the lower current.

In the report of Dr. Moesta, who observed the total solar eclipse of November 30, 1853, to the south of Pisco in Peru, it is especially worthy of remark that, fifteen minutes prior to the sun being totally eclipsed, the yet existing crescent of the sun became suddenly "invested with a dark rose-coloured nebular substance." The intensity of this covering kept on increasing, so that at last the edges of the sun and moon could only be indistinctly made out. After the commencement of the total eclipse, there was seen on the western edge of the moon a rose-coloured elevation which had a southerly motion, and which disappeared in a minute and twenty seconds, its colour having first passed to orange and then to yellow. In addition thereto there were observed on the northerly edge two *completely dark* protuberances, which were without doubt masses of vapour of greater density. The sketch made by Moesta is shown in fig. 7.

On the occasion of the total solar eclipse of September 7, 1858, which was observed by a commission of astronomers at Paranagua in Brazil, there were seen on the eastern edge of the moon two white prominences with a black border, and one prominence entirely white, which were apparently hidden by the moon as it advanced: three prominences of a reddish colour made their appearance by degrees on the western edge, and they would seem to have increased slowly in size. Here also, therefore, we should have to assume a motion of the vapour from east to west.

Among the meteorological notices, we find it stated that about three-quarters of an hour after the total eclipse, clouds approached the sun from the eastward, which harmonizes with what we have assumed above; it must not, however, be overlooked that in the morning rain-clouds came up from the west; so that here, again, the direction of the current of the air remains undecided.

It would be easy to gather from the reports before us on the total solar eclipses that have been observed, many more additional hints that coincide with and support my explanation of the red prominences; but I confine myself to what has been stated above, and this the more from my being of opinion that decisive facts have first to be arrived at by *future* observations. It will be especially advisable to follow up the masses of vapour that condense in the cone of shadow,—and to do so not only at the moon's edge, but likewise in the aureole as far outwards as the light extends, determining at the same time their magnitude and the direction of their motion; perhaps, indeed, it might be practicable to gain some information upon this head immediately previous to the disappearance of the sun, and immediately subsequent to its reappearance. The visibility of the masses of vapour depends mainly on their density; and that they are not unfrequently of considerable density is proved by the occurrence of prominences that are black, or which at least are bounded by a black border.

LVI. *On the Vertical Currents of the Atmosphere. By* Henry Hennessy, *F.R.S., M.R.I.A., Professor of Natural Philosophy in the Catholic University of Ireland* [*].

1. IT has long been recognized that, although currents of wind in a direction nearly parallel to the horizon are those which usually prevail, the atmosphere is frequently subjected to vertical and oblique motions among its particles.

Under favourable conditions these motions may acquire such a development as to force themselves upon the attention of observers, and thus become objects for meteorological inquiry. The interesting researches of M. Fournet upon the vertical currents of mountains, appear to have arisen from the opportunities enjoyed by that physicist of studying such phænomena among the Alps. Among the deep ravines and valleys, as well as along the elevated slopes and escarpments of the Alps, a regular periodicity in the action of vertical winds has frequently been observed during the course of twenty-four hours, which has led to the conclusion that their development depends upon changes

of temperature resulting from the presence and absence of the sun. As it is now well established that the distribution and changes of temperature in these islands are dependent upon other influential causes besides the direct action of the sun*, we cannot, in general, expect to find in our climate a similar diurnal periodicity so distinctly defined as that observed in the centre and south of Europe. Here, as well as on the Continent, mountains are favourable to the production of inequalities of temperature, moisture, and density among the aërial strata, which thus become liable to a multitude of disturbances, and especially to the action of vertical currents. It seems to follow, that in mountainous countries vertical currents have well-marked relations with the changes of the weather.

If, as usually happens, lakes exist among the mountains, the mysterious occurrence called the " bore " is also thus explained. The circumstance that the suddenly-formed wave thus designated always proceeds from a side of the lake bordered by steep mountains, immediately suggests such an explanation. Although a similar idea has occurred to other inquirers, I may be permitted to refer to an instance where a demonstration was presented by me† of the efficiency of vertical currents in producing the " bore " on the surface of one of our Irish lakes. The fact that such a sudden wave usually preceded a change of the weather in the district surrounding the lake, led me to think that the study of the effective cause of the bore itself might become of importance in meteorology. But to do this, we should possess means for observing the actual direction and, if possible, the force of the atmospheric currents.

2. Hitherto all instruments which had been employed for observing the wind were devised exclusively with reference to its horizontal direction and intensity, from the simple wind-vane to the most finished anemometer‡. I have attempted to modify the ordinary vane so as to make it an indicator of the actual direction of the current, both in altitude and azimuth. Instead of the fixed surface against which the wind impinges in ordinary vanes, I had a disc suspended at the tail of the vane capable of rotating on an axis perpendicular to the line of direction of the instru-

* See Phil. Mag. for October 1858; also a letter from the author to Major-General Sabine, " On the Influence of the Gulf-stream on the Winters of the British Islands," Proceedings of the Royal Society, vol. ix. p. 324.

† In a letter to the Rev. T. R. Robinson, D.D., of Armagh. See Proceedings of the Royal Irish Academy, vol. vi. p. 279.

‡ Some time after the anemoscope had been devised, my attention was called by my friend the Rev. Dr. Robinson, to a passage among the notes to Dr. Darwin's poem of the ' Botanic Garden,' wherein the writer indicates such an instrument; but he seems never to have realized this idea, and the apparatus which he proposed was essentially different from mine.

ment. A pair of flanges were attached to this disc in such a manner that, when the whole was at rest and the air free from motion, the flanges would be horizontal. With perfectly horizontal currents, the flanges would still continue in the same position, although the head of the vane would as usual move about in azimuth. But if a current happened to be inclined to the horizon, the flanges would be pressed upwards or downwards, showing the direction and amount of the inclination, precisely as the position of the head or tail of the ordinary vane shows the direction and inclination of a current with reference to the meridian. When we know the inclination of a given current to the horizon, we can readily estimate its absolute force from its horizontal force, as can be easily shown.

3. Let the origin of coordinates be at the centre of the axis of the vertical disc; $y\,dx$ will represent an element of the area of the flange. Let θ represent the angle of inclination of the flange, H the pressure exercised by the wind in a horizontal direction upon a square unit of surface, and V the vertical pressure exercised upon a similar unit. The entire moment of the horizontal forces acting on the entire flange will be

$$\text{H} \int \sin\theta\, xy\,dx,$$

and the moment of the vertical forces will be

$$\text{V} \int \cos\theta\, xy\,dx.$$

Both of these moments tend to cause a rotation of the disc, but in contrary directions: hence when the disc is in equilibrium they must be equal, and therefore, because θ is independent of x and y, we shall have

$$\text{H} \sin\theta = \text{V} \cos\theta, \quad \text{V} = \text{H} \tan\theta; \quad \cdot \quad \cdot \quad \cdot \quad (1)$$

and if we write F for the absolute force of the wind, we shall have

$$\text{F} = \text{H} \sec\theta. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (2)$$

Hence it follows, that if we can observe the absolute direction of the wind, we can estimate its vertical force as well as its absolute intensity without any special instrument, using the results obtained by the existing anemometers which give the horizontal intensity.

4. A wind-vane or *anemoscope*, capable of showing the absolute direction of an atmospherical current, having been constructed in accordance with my directions, I proceeded to make some observations during the months of June, July, and August, 1857. It was placed on the top of a strong mast, about twenty-six feet in height. The mast was fixed near the end of a large garden, far from buildings. As my first series of observations

were intended to be merely provisional, I did not make them at specific fixed hours, but at such times as presented disturbances in the atmosphere, or which afforded sufficient leisure for continued attention. A journal was kept, from which I make the following extracts. Before doing so, it is proper to remark that by the term "vertical currents" in these extracts, as well as in the title of this paper, I do not mean currents actually perpendicular to the horizon, but rather oblique currents with an upward or downward tendency.

"June 28, 7 A.M.—Air perfectly still, flanges horizontal, head of vane towards the east. 7.30 A.M.—Breeze with slight vertical currents until after 8. The currents were upward from the ground. The flanges were often perfectly horizontal, and their mean angle of inclination was small. About 10 A.M., a few fine scattered clouds (cirro-cumuli) were observed to move in a direction contrary to the wind as observed near the earth.

"From 3 P.M. to 3.45.—Wind extremely gentle from E.S.E. Upward current, angle of inclination estimated at about 5°. The upward currents often continued for several minutes together. The angle was sometimes almost imperceptible. The sky became gradually overcast towards evening.

"June 30, 10 A.M.—Sky completely overcast, strong wind from E.S.E., rapid oscillations of the disc during the greater part of the day. About 6 P.M., the wind blew in violent gusts from the east, and the disc showed alternations of upward and downward currents with occasional short intervals. These observations led me to conclude that rapid currents of air cannot generally advance with the same steadiness as currents of water; the greater mobility and elasticity of the former fluid probably allow its movements to easily acquire a species of undulation. Thus we may account for the motions of the branches of trees, which generally swing backwards and forwards, showing rapid variations in the intensity of the wind. During breezes composed of a succession of strong sudden gusts, it was difficult to estimate the inclination of the flanges, as each fresh impulse drove the flange beyond the angle due to the pressure, and before it had been sufficiently long oscillating about its true position to allow a correct observation, a fresh gust would perhaps drive it in a different direction.

"July 1, 9 A.M.—Wind N.E., strong breeze with vertical currents. The position of the flanges was sometimes steady for many minutes, with a very small inclination; upward currents appeared to predominate in duration.

"July 2, before 9 A.M.—Air still and warm, head of vane directed to S.E. After 9 a gentle breeze from E. and E.S.E., with an upward tendency. The disc remained steady at a small angle, sometimes for two minutes together. Towards noon the

disc was more steadily upward, while the breeze still continued. The clouds were observed to move from W.N.W. At 6.30 P.M., a gentle breeze from W.S.W.; sky covered with light clouds; steady upward tendency of the current; very little waving of trees. The flanges sometimes retained the same inclination for a quarter of an hour. 8.30 P.M., wind more brisk from the west, but the disc still steady; sky beginning to become overcast.

"July 3, 8 A.M.—Wind S.W. and S.; air filled with heavy clouds, floating at comparatively short distances from the earth. Strong breeze with alternate up and down currents, the downward currents lasting but for very short periods. 9.15 A.M., wind S.S.E. with light rain. Just *before* the rain the downward currents became more prominent, the clouds moved from S.W. 10.30 A.M., wind S.S.W. with alternate upward and downward currents.

" July 5.—Fine morning, clear sky, with a few scattered cumuli; gentle breeze from S.W., alternating currents upward predominant. 2 P.M.—Cloudy sky, with the air almost still; slight vertical currents. Rain from four to seven o'clock. 9 P.M.—Wind N.N.W., clearing the sky; temperature rapidly falling, with downward currents. Towards midnight the sky was almost perfectly clear, and the wind more westerly.

"July 6, 9 A.M.—Very strong breeze from N.W., with vertical currents and rain. The alternations were sometimes rapid, and the apparent angle of inclination very great; the disc rarely continued steady in an inclined position, although it sometimes remained for long intervals in a perfectly horizontal position, with a strong wind. Rain appeared to produce no remarkable effect on the flanges, for it seemed to be shaken or blown off. About 3 P.M., the wind was strong and steady from N.N.W.; the movements of the flange were as follows during the course of a few minutes :—Downward, $1\frac{1}{2}$ minute; upward, $\frac{1}{2}$ min.; level, $\frac{1}{4}$ min.; oscillating, $\frac{3}{4}$ min.; down, $\frac{1}{4}$ min.; up, $\frac{1}{2}$ min.; oscillating, $\frac{1}{2}$ min.; level, $\frac{1}{2}$ min.; up, $\frac{1}{4}$ min.; oscillating, $\frac{1}{4}$ min.; level, $\frac{1}{4}$ min.; up, 1 min.; down, $\frac{1}{4}$ min. The air was gradually filling with broken masses of cumulo-stratus clouds. As they appeared to approach the earth, downward oscillations of the flange became more manifest. Approaching four o'clock the wind blew irregularly, with violent and sudden gusts of short duration. At $6\frac{1}{2}$ P.M., a strong breeze, with currents having a downward tendency; towards seven the sky became a little more clear, and the currents appeared to be alternately upward and downward, with short intervals of 10 or 12 seconds. At 7.15 P.M. the wind was from N.W., with alternate currents, the upward predominating, while the sky was becoming perceptibly more clear.

The upward currents were decidedly longer in duration than at 6 P.M.

"9½ P.M.—Wind still from N.W.; upward currents, with alternating currents at intervals of about one minute.

"July 11.—Wind W. A beautiful day, with a few light clouds scattered over the sky. During the afternoon, up to 5 P.M., a strong breeze, with very decided upward currents. At short intervals the disc oscillated, showing a downward tendency.

"July 14.—Before 9 A.M., the wind was E.S.E.; a moderate breeze with downward tendency. Light clouds were observed to move in a direction opposed to the wind at the earth's surface. 10.30 A.M., wind S.E.; an increase of clouds (cumuli); both vane and disc were oscillating; downward tendency of currents was marked. At 1 o'clock in the afternoon, a fog was seen out at sea, which, as it approached the shore, ascended in clouds over Howth.

"August 6, 10 A.M.—Wind N.E.; alternate currents, downward predominating. The sky was covered with light clouds, and the temperature comparatively low.

"August 20.—An extremely fine and warm day, with a clear sky. The air was nearly still; and the disc continued to indicate faint and steady upward currents; for the flange continued at an upward inclination of a few degrees for long intervals, sometimes exceeding one hour. The movements of smoke that could be observed at the same time showed a similar tendency.

"August 21, 7 A.M.—Wind E.S.E., with no vertical currents; after 8, the disc began to move, and the flange was sometimes inclined upwards at a very small angle. It frequently remained perfectly level, although a very perceptible breeze was blowing. After 10 A.M. the upward tendency became more manifest, and it generally remained for long intervals inclined at an angle of from about 5° to 8°.

"August 24, 5 P.M.—*Before* and during a heavy shower the disc exhibited the presence of downward currents.

"September 3, 8 A.M.—Wind blowing in sudden gusts from N.E.; the disc showed vertical currents, chiefly with a downward tendency; rain followed at about half-past nine."

5. The few results which were thus recorded seem to show that the study of the non-horizontal motions of our atmosphere is desirable, not only among mountainous districts, but that it may form a portion of our general inquiries under all local circumstances whatever. It appears that the wind rarely blows parallel to the surface of the earth, and that the air, while in rapid motion, is always undergoing a process of undulation, whereby the direction of the axis of a current at any point above the earth is changed alternately, so as to be more or less inclined upwards or

downwards, just as the direction of the wind in azimuth is frequently observed to slightly oscillate about its mean position. We may conclude, therefore, from sect. 3, that the absolute force of the wind is always a little greater than its horizontal intensity, as exhibited by the anemometers.

While such an undulatory motion of the atmospherical currents may be generally due to the elasticity of the air and the mechanical influence of terrestrial irregularities, many of my observations were such as to clearly show the existence of true upward and downward currents. In no other way can we account for the steady inclination of the flanges of the anemoscope at times when scarcely any horizontal wind was perceptible. When true upward currents were prevalent, the temperature of the air was usually increasing and the weather fine. Downward currents seemed to be usually preceded or accompanied by a sudden decrease of temperature, and these currents themselves usually preceded rain or unfavourable weather. Regular alternations of both classes of currents were usual about noon or the forenoon of clear days. The explanation of the last circumstance is extremely simple. It depends upon the manner in which the atmosphere acquires the greatest part of its heat during the day. A small portion of the solar heat is immediately absorbed in passing through the air, but the greater part reaches the ground, whence it is imparted to the atmosphere immediately touching it. The air so heated expands, and consequently, from its reduced density, it tends to penetrate upwards in currents through the overlying strata, which at the same time fall downwards to fill up the vacancies. A species of convection, analogous to that seen in a boiling or heated mass of liquid, is thus developed in the air. The trembling of the air, often noticed over steam-boilers, close to the chimneys of steam-vessels, and even on walls and gravelled walks heated by the mid-day sun, is undoubtedly due to the same minute and rapid currents which take part in this process of aërial convection.

6. That there are more important vertical currents engaged in promoting exchanges between the upper and lower strata of the atmosphere, within a short distance from the earth, appears manifest from experiments made by me in May 1858 *. Thermometers were suspended at different heights, and under different circumstances of exposure to the supposed currents. On days when the sky was clear, and when, consequently, the direct influence of the sun in heating the ground was most decided, observations were made every minute, and sometimes every half minute, during short intervals. More or less rapid oscillations

* Report of the British Association for 1858. Transactions of Sections, p. 36.

of the mercury were observed. In thermometers freely exposed to the air, the mercury sometimes rose or fell three degrees Fahrenheit in three minutes. The longest fluctuations did not occupy more than six minutes. The fluctuations diminished the more the thermometers were protected from the influence of the currents of air.

A further confirmation of these results is found in the Report of the Director of the Radcliffe Observatory at Oxford, relative to the meteorological observations during the year 1857.

The thermometrical curves exhibited a remarkable serration during the day-time of the most brilliant months of the year. This serration entirely ceased during the winter, and on gloomy days at every season : its intensity seemed to increase with sunshine. It is readily explained by the action of small atmospherical currents alternately ascending and descending, the former producing a sudden and brief elevation of the mercury, and the latter a sudden and short depression. The curves referred to were obtained at the Radcliffe Observatory, by a very beautiful application of the waxed-paper photographic process ; and the results here noticed would probably never have been exhibited by the ordinary observations at stated hours. I cannot refrain from remarking that the success which has attended this portion of the application of photographical registration to meteorology, has much increased my confidence in its trustworthiness, while it has inspired a feeling of deep regret at the loss which science has sustained by the death of Mr. Johnson, to whose able management and indefatigable labours these and many other results are mainly due.

7. To such small currents we may attribute whirlwinds of more or less magnitude, from those which we often observe on dusty roads, to the grand and frequently dangerous phænomena of the desert. Mr. Belt, who writes in the Philosophical Magazine for January 1859, presents some very instructive observations on this subject. The ascending currents over dry ground in the interior of Australia, were frequently observed by him to carry leaves and dust to the upper regions of the atmosphere. Often, when travelling over parched plains, this observer saw the air quivering over the hot ground as if close to the wall of a furnace ; suddenly a miniature storm arises, and after a few minutes' violence it as suddenly ceases, while the quivering of the air is no longer seen and the atmosphere does not feel oppressive. All these phænomena are obviously the results of more or less energetic interchanges between masses of air possessing different temperatures. The process of convection in this case is not of a gentle and gradual nature, but takes place with fitful violence. The phænomena here referred to seem to present on a small

scale the principal features of cyclonic storms and hurricanes. These are always preceded by inequalities of temperature in the regions where they occur; and it is extremely probable that such inequalities take place in a vertical as well as in a horizontal direction. The distribution of watery vapour must at the same time be affected; and this would again react upon the equilibrium of the atmosphere, so as to favour the existence of ascending and descending currents. The rapid oscillations of the barometric column which usually precede hurricanes are thus doubtlessly connected, not only with variations in the statical pressure, but also with the irregular influence of vertical and oblique currents, which at such times disturb the equilibrium of the atmospherical column over the barometer.

8. The duration and energy of many of the vertical currents which came under my observation, were such as to show that currents of a greater order than those which take place by the influence of the heated ground immediately beneath are sometimes developed among the overlying atmospheric masses. Such currents being of much greater magnitude than those which would account for the rapid fluctuations of the thermometer already noticed, we may refer to them not only great interchanges of temperature in different strata of the atmosphere, but also a very efficient part in the production of ordinary winds. If an extensive portion of the earth's surface becomes more heated than other surrounding portions, the air will ascend and overflow above the cooler air resting upon the unheated surfaces. The cold air at bottom will at the same time tend to rush inwards, so as to fill up the vacuum which the ascending currents would have left above the surface of the heated ground. As the air that overflows above does not rush into a vacuum, but penetrates and mingles with masses of cooler air possessing nearly the same density, its progress is considerably retarded, while at the same time some of the vapour which it may contain is condensed so as to assume a vesicular cloudy form. A corresponding retardation in the motions of the air rushing in from the colder to the warmer surface below, is also produced from the resistance of the air lying over the latter. The production of sea- and land-breezes furnishes a complete and instructive illustration of these remarks. Many of the upward currents which I observed with the anemoscope during the summer mornings, were undoubtedly the precursors of the sea-breeze. Such currents continue to accompany the production of the land- and sea-winds in a manner that I have sometimes been able to observe by the smoke of steam-vessels near the coast. Thus, on a warm day in June 1857, I observed the simultaneous existence of the sea-breeze at Kingstown and a slight motion of a few light clouds from the interior towards the

coast. A steam-ship far out at sea was proceeding towards England, and the smoke was drawn by the gentle breeze into a streamer extending for miles behind the boat. The streamer of smoke appeared straight and perfectly horizontal over the surface of the water, until it arrived at a point about a quarter of a mile from the Hill of Howth, when it rose upwards with a gracefully-curved outline, and it appeared to be gradually diffused in the air situated vertically over the hill.

The influence of vertical and oblique currents in the atmosphere is not only thus manifest in the comparatively limited and local phænomena of sea- and land-breezes, mountain winds, and whirlwinds, but it has also been appealed to in order to explain the circulation of the great winds of the earth. Thus Maury, in his attempt to exhibit the general laws of the great winds, presents a diagram in which ascending and descending currents are distinctly indicated over different regions of the globe. Their agency is also appealed to by other inquirers; and their principal seats of action seem to be indicated as the calm regions, that is to say, the regions where horizontal winds blow with least intensity. Observations with the aid of the anemoscope in the regions of equatorial and tropical calms would therefore probably serve to test the accuracy of the general views here alluded to. The systematic study of the non-horizontal movements of the atmosphere has scarcely been commenced; but what little knowledge we possess of such movements shows that they are so closely connected with some of the most important phænomena of the weather, that their further investigation is certain to be attended with interesting and valuable results.

LVII. *On the Percussion of Bodies.* By M. POINSOT[*].

[Continued from vol. xviii. p. 259.]

CHAPTER V.

1. IN the very special cases hitherto treated[†], we supposed that the motion of the body was due to the impulse of a *single force* P having a certain direction, and we merely determined the percussion Q which the body was capable of producing against a fixed point presented to it in a peculiar manner.

It now remains to treat the general question where the motion of the body is due to the action of *any given forces whatever*, and where the percussion is required which this body can produce, by any one of its points, against any fixed obstacle which it may encounter.

[*] From Liouville's *Journal*, December 1859.
[†] See Phil. Mag. vols. xv. and xviii.

General Problem.

2. *A free solid body being animated by given forces, any one of its points* C *suddenly encounters a fixed point which compels the body to change its motion; required the direction and magnitude of the percussion which will be produced upon this obstacle.*

3. The solution is not difficult to find; for if we represent the percussion on the fixed point by Q, it is evident that a force $-Q$, equal and contrary to Q, applied to the body at the moment of the shock, would at that moment precisely destroy the velocity of the point C.

In order to obtain the equations of the problem, therefore, it will suffice to express the condition that, under the influence of all the *given* forces, and of the *unknown* force $-Q$ applied at the point C, this point of the body acquires a velocity equal to zero; this sole condition being developed, will supply all that is necessary for the determination of the magnitude and direction of the required percussion Q.

Development of the Solution.

4. Let us make the three *principal* axes of the body which pass through its centre of gravity G our coordinate axes; and represent by m the mass of the body; by $m\alpha^2$, $m\beta^2$, $m\gamma^2$ its three principal moments of inertia; and by x, y, z the coordinates of the point C.

The *given* forces may be reduced to three forces,

$$X_0, \quad Y_0, \quad Z_0,$$

directed along the three axes, and to three couples

$$L_0, \quad M_0, \quad N_0,$$

in planes perpendicular to these axes.

Similarly the *unknown* force $-Q$, applied at the point C, may be decomposed into three forces,

$$X, \quad Y, \quad Z,$$

applied at the centre of gravity G and directed along the three axes, and into three couples around these axes, whose moments will be expressed by

$$Yz-Zy, \quad Zx-Xz, \quad Xy-Yx.$$

5. The system of all the forces is thus reduced to the three forces

$$X_0+X, \quad Y_0+Y, \quad Z_0+Z,$$

applied at the centre of gravity along the axes, and to the three couples

$$L_0+Zy-Yz, \quad M_0+Xz-Zx, \quad N_0+Yx-Xy$$

in planes perpendicular to these axes.

6. We have now to find the *velocity* of the particular point C under the influence of these forces and couples.

7. In the first place, the three forces applied to the centre of gravity impart to every point of the body, and therefore to C, the velocities $\dfrac{X_0+X}{m}, \quad \dfrac{Y_0+Y}{m}, \quad \dfrac{Z_0+Z}{m}$ . . . . . (1)

along the coordinate axes of x, y, z respectively.

8. In the second place, the three couples tend to cause the body to turn around these axes with angular velocities whose respective values are

$$p=\frac{L_0+Zy-Yz}{m\alpha^2}, \quad q=\frac{M_0+Xz-Zx}{m\beta^2}, \quad r=\frac{N_0+Yx-Xy}{m\gamma^2}. \quad (2)$$

But it will easily be seen that, in virtue of these three rotations, the point C will have, in the directions of the axes of x, y, z, the velocities

$$qz-ry, \quad rx-pz, \quad py-qx \quad . \quad . \quad . \quad . \quad (3)$$

respectively. Consequently, by adding these velocities to the three preceding ones, and representing by $\dot{x}, \dot{y}, \dot{z}$ the total velocities of the point C along the coordinate axes of x, y, z, we shall have
$$\dot{x}=\frac{X_0+X}{m}+qz-ry,$$
$$\dot{y}=\frac{Y_0+Y}{m}+rx-pz,$$
$$\dot{z}=\frac{Z_0+Z}{m}+py-qx;$$

whence, by substituting the values of p, q, r as given in (2), we deduce the three equations

$$\left.\begin{array}{l}\dot{x}=\dfrac{X_0+X}{m}+\dfrac{z(M_0+Xz-Zx)}{m\beta^2}-\dfrac{y(N_0+Yx-Xy)}{m\gamma^2},\\[2mm]\dot{y}=\dfrac{Y_0+Y}{m}+\dfrac{x(N_0+Yx-Xy)}{m\gamma^2}-\dfrac{z(L_0+Zy-Yz)}{m\alpha^2},\\[2mm]\dot{z}=\dfrac{Z_0+Z}{m}+\dfrac{y(L_0+Zy-Yz)}{m\alpha^2}-\dfrac{x(M_0+Xz-Zx)}{m\beta^2}.\end{array}\right\} \quad . (A)$$

9. These are the equations which furnish at once the components $\dot{x}, \dot{y}, \dot{z}$ of the velocity j imparted to the point C of the body by the given forces which animate the same, combined with the unknown force $-Q$ supposed to be applied at the point C itself.

10. But the force $-Q$ being properly chosen, the velocity j of the point C, and consequently each of the components $\dot{x}, \dot{y}, \dot{z}$ of this velocity will become zero.

Putting $\dot{x}=0, \dot{y}=0, \dot{z}=0$, therefore, the formulæ (A) give

the following three equations :—

$$\left.\begin{array}{l}(\beta^2 y^2 + \gamma^2 z^2 + \beta^2\gamma^2)\mathrm{X} - \beta^2 xy\mathrm{Y} - \gamma^2 xz\mathrm{Z} = -\beta^2\gamma^2\mathrm{X}_0 - \gamma^2 z\mathrm{M}_0 + \beta^2 y\mathrm{N}_0, \\ -\alpha^2 xy\mathrm{X} + (\alpha^2 x^2 + \gamma^2 z^2 + \alpha^2\gamma^2)\mathrm{Y} - \gamma^2 yz\,\mathrm{Z} = -\alpha^2\gamma^2\,\mathrm{Y}_0 - \alpha^2 x\mathrm{N}_0 + \gamma^2\, z\mathrm{L}_0, \\ -\alpha^2 xz\mathrm{X} - \beta^2 yz\mathrm{Y} + (\alpha^2 x^2 + \beta^2 y^2 + \alpha^2\beta^2)\mathrm{Z} = -\alpha^2\beta^2\,\mathrm{Z}_0 - \beta^2 y\mathrm{L}_0 + \alpha^2 x\,\mathrm{M}_0,\end{array}\right\} \cdot (4)$$

from which the components X, Y, Z of the force $-\mathrm{Q}$ may be found. By changing the signs of these components, we shall, of course, obtain those of the required percussion Q.

Resolution of the preceding Equations.

11. The three equations (4) being of the first degree in X, Y, Z, may at once be solved by known formulæ. Thus if X, Y, Z be represented by the three fractions

$$\mathrm{X} = \frac{\mathrm{N}_x}{\mathrm{D}}, \quad \mathrm{Y} = \frac{\mathrm{N}_y}{\mathrm{D}}, \quad \mathrm{Z} = \frac{\mathrm{N}_z}{\mathrm{D}},$$

we shall have for the common denominator the value

$$\frac{\mathrm{D}}{\alpha^2\beta^2\gamma^2} = (x^2 + y^2 + z^2)(\alpha^2 x^2 + \beta^2 y^2 + \gamma^2 z^2) + \alpha^2(\beta^2 + \gamma^2)x^2 \\ + \beta^2(\alpha^2 + \gamma^2)y^2 + \gamma^2(\alpha^2 + \beta^2)z^2 + \alpha^2\beta^2\gamma^2,$$

and for the numerators the values

$$\frac{\mathrm{N}_x}{\alpha^2\beta^2\gamma^2} = -[x^2(\alpha^2 x^2 + \beta^2 y^2 + \gamma^2 z^2) + \gamma^2(\alpha^2 x^2 + \beta^2 y^2) + \beta^2(\alpha^2 x^2 + \gamma^2 z^2) + \alpha^2\beta^2\gamma^2]\mathrm{X}_0 \\ - xy(\alpha^2 x^2 + \beta^2 y^2 + \gamma^2 z^2 + \alpha^2\beta^2)\mathrm{Y}_0 - xz(\alpha^2 x^2 + \beta^2 y^2 + \gamma^2 z^2 + \alpha^2\gamma^2)\mathrm{Z}_0 \\ + xyz(\beta^2 - \gamma^2)\mathrm{L}_0 - z(\alpha^2 x^2 + \gamma^2 y^2 + \gamma^2 z^2 + \alpha^2\gamma^2)\mathrm{M}_0 \\ + y(\alpha^2 x^2 + \beta^2 y^2 + \beta^2 z^2 + \alpha^2\beta^2)\mathrm{N}_0,$$

$$\frac{\mathrm{N}_y}{\alpha^2\beta^2\gamma^2} = -xy(\alpha^2 x^2 + \beta^2 y^2 + \gamma^2 z^2 + \alpha^2\beta^2)\mathrm{X}_0 \\ - [y^2(\alpha^2 x^2 + \beta^2 y^2 + \gamma^2 z^2) + \alpha^2(\beta^2 y^2 + \gamma^2 z^2) + \gamma^2(\alpha^2 x^2 + \beta^2 y^2) + \alpha^2\beta^2\gamma^2]\mathrm{Y}_0 \\ - yz(\alpha^2 x^2 + \beta^2 y^2 + \gamma^2 z^2 + \beta^2\gamma^2)\mathrm{Z}_0 \\ + z(\gamma^2 x^2 + \beta^2 y^2 + \gamma^2 z^2 + \beta^2\gamma^2)\mathrm{L}_0 + xyz(\gamma^2 - \alpha^2)\mathrm{M}_0 \\ - x(\alpha^2 x^2 + \beta^2 y^2 + \alpha^2 z^2 + \alpha^2\beta^2)\mathrm{N}_0,$$

$$\frac{\mathrm{N}_z}{\alpha^2\beta^2\gamma^2} = -xy(\alpha^2 x^2 + \beta^2 y^2 + \gamma^2 z^2 + \alpha^2\gamma^2)\mathrm{X}_0 \\ - yz(\alpha^2 x^2 + \beta^2 y^2 + \gamma^2 z^2 + \beta^2\gamma^2)\mathrm{Y}_0 \\ - [z^2(\alpha^2 x^2 + \beta^2 y^2 + \gamma^2 z^2) + \alpha^2(\beta^2 y^2 + \gamma^2 z^2) + \beta^2(\alpha^2 x^2 + \gamma^2 z^2) + \alpha^2\beta^2\gamma^2]\mathrm{Z}_0 \\ - y(\beta^2 x^2 + \beta^2 y^2 + \gamma^2 z^2 + \beta^2\gamma^2)\mathrm{L}_0 \\ + x(\alpha^2 x^2 + \alpha^2 y^2 + \gamma^2 z^2 + \alpha^2\gamma^2)\mathrm{M}_0 + xyz(\alpha^2 - \beta^2)\mathrm{N}_0.$$

12. If, then, in the expression

$$\mathrm{Q} = \sqrt{\mathrm{X}^2 + \mathrm{Y}^2 + \mathrm{Z}^2} = \frac{\sqrt{\mathrm{N}_x^2 + \mathrm{N}_y^2 + \mathrm{N}_z^2}}{\mathrm{D}}$$

the preceding values of N_x, N_y, N_z, D be substituted, the required percussion Q will be obtained as a function of the data of the problem; that is to say, of the three *arms* α, β, γ of inertia of the body, of the given forces and couples X_0, Y_0, Z_0, L_0, M_0, N_0 which animate it, and of the three coordinates x, y, z of the point C of the body where the obstacle is presented.

13. The form of the above expressions shows that, in virtue of the applied forces and couples, the actual percussion of the body is composed of the percussions which would be produced upon the same point if each of these given forces and couples acted separately, and this should clearly be the case.

It may also be remarked, that all these general formulæ may be verified by applying them to the particular cases treated in the first two chapters.   By so doing the reader may convince himself of the perfect accordance between our results.

In conclusion, it will be well to add a short remark with respect to the precise nature of the obstacle considered in the problem which has just been solved.   It is simply a *fixed point* which is supposed to be capable of suddenly and totally arresting the point C of the body which strikes it ; that is to say, of retaining it *for an instant* in the same position in space, just as if this point C of the body had, for an instant, fallen into the interior of a hollow and resisting sphere of infinitely small radius. It must also be borne in mind that, after the shock, this obstacle is supposed to disappear entirely ; for after the shock the body merely retains a rotation around a spontaneous axis passing through C, and therefore becomes incapable of striking an obstacle presented at this point.

By means of its new motion, however, the body is capable of striking with any other point C' ; and the new percussion may be found from the same formulæ on replacing the old forces by the new ones : we are thus led naturally to the theory of the singular motions known as *ricochets*.

LVIII. *On the Dynamical Theory of Gases.*
By Professor CLAUSIUS.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE January Number of your Journal contains a very valuable paper by Professor Maxwell, entitled " Illustrations of the Dynamical Theory of Gases," in which occurs (see Prop. X.) a result opposed to an assertion made by me in a previously published paper*.   Having waited in vain for the pro-

* Phil. Mag. February 1859.

mised continuation of Prof. Maxwell's paper, I beg now to forward my reply.

In my paper I consider the following question: a molecule μ of a gas moves with a certain velocity in a space which already contains many other molecules m, m_1, m_2 ..., and in so doing occasionally strikes against and rebounds from the latter; required the number of collisions made by μ in the unit of time, or what is equivalent, the magnitude of the mean length of path between two consecutive points of collision. In my solution I confined myself to the case where the molecule μ moves, and the others m, m_1, ... remain at rest; but at the same time I asserted that in the case where the latter molecules also move *with the same velocity as* μ, the number of collisions increases in the ratio of $1 : \frac{4}{3}$. I did not prove this assertion, because for the object I then had in view it was not necessary to enter into such particulars. Since Prof. Maxwell, however, in his treatment of the same subject, arrives at the ratio $1 : \sqrt{2}$ instead of $1 : \frac{4}{3}$, I feel myself called upon to prove the accuracy of my former statement.

Let us first assume that μ alone moves, whilst m, m_1, m_2 ... remain at rest; and let v be the velocity of μ, N the number of molecules at rest in the unit of space, and s the magnitude to which the distance between the centres of μ and any other molecule must be reduced before a collision can occur. The number of collisions during the unit of time will then be

$$v \pi s^2 \text{N}.$$

If we now assume that the molecules m, m_1, m_2 ... also move, we must replace the *actual* velocity v by the *relative* velocities of the molecule μ with respect to the molecules m, m_1, m_2 ...; and since these relative velocities differ from each other, the arithmetical mean of all their values must be taken. Representing this mean by r, the number of collisions will be

$$r \pi s^2 \text{N},$$

and consequently the ratio of the number of collisions in the two cases will be $v : r$.

Thus far Prof. Maxwell and I agree, so that it will not be necessary to enter here into the demonstration of the above formulæ; we differ only in the determination of the mean value r. Let u be the velocity of any molecule m, and ϑ the angle between the direction of its motion and that of the molecule μ; the relative velocity between μ and m will then be

$$\sqrt{u^2 + v^2 - 2uv \cos \vartheta}.$$

When the molecules m, m_1, m_2 ... all move with the same velo-

city, in other words, when u is constant, and ϑ alone variable from one molecule to another, the mean value can be easily calculated. According to Prof. Maxwell, the value in question is

$$r = \sqrt{u^2 + v^2};$$

whence, when $u = v$, follows $r = v\sqrt{2}$. This value is incorrect, however, as will be seen from the following considerations.

Since all directions are equally probable for the molecules m, $m_1, m_2, \ldots$, the number of those whose lines of motion make angles between ϑ and $\vartheta + d\vartheta$ with the line in which μ moves will have to the whole number of molecules the same ratio that a spherical zone with the polar angle ϑ and the breadth $d\vartheta$ has to the whole surface of the sphere, in other words, the ratio

$$2\pi \sin \vartheta \, d\vartheta : 4\pi.$$

The number of such molecules in the unit of volume is consequently

$$N \cdot \tfrac{1}{2} \sin \vartheta \, d\vartheta.$$

In order to obtain the required mean value r, the last expression must be multiplied by the relative velocity which corresponds to it, the product integrated between the limits o and π, and the integral divided by N. Hence

$$r = \tfrac{1}{2} \int_0^\pi \sqrt{u^2 + v^2 - 2uv \cos \vartheta} \cdot \sin \vartheta \, d\vartheta.$$

This gives at once

$$r = \frac{1}{6uv}\left[(u^2 + v^2 + 2uv)^{\frac{3}{2}} - (u^2 + v^2 - 2uv)^{\frac{3}{2}} \right],$$

whence we may deduce

$$r = v + \tfrac{1}{3}\frac{u^2}{v}, \text{ when } u < v,$$

and

$$r = u + \tfrac{1}{3}\frac{v^2}{u}, \text{ when } u > v.$$

When $u = v$, both results coincide in value with

$$r = \tfrac{4}{3}v,$$

and thus verify my assertion.

I remain, Gentlemen,

Yours respectfully,

Zurich, April 25, 1860.   R. Clausius.

LIX. *On the Law of the Wave-lengths corresponding to certain points in the Solar Spectrum. By* Mungo Ponton, *F.R.S.E.**

THE first attempt to find a law regulating the wave-lengths corresponding to definite points in the solar spectrum was made by Sir Isaac Newton, who chose for investigation the boundary lines of the seven colours, being the only determinate points then known. He adopted as the basis of his law a series deduced from certain divisions of the musical chord. The same series, however, may be obtained in a more palpable manner from the following geometrical construction.

Round the point O describe a circle, and inscribe the equilateral triangle F A D. Draw the diameter A E bisecting the tri-

angle, and perpendicular to this diameter draw another, X Y. Draw O B bisecting the arc A Y. Make A C equal to the side of a pentagon inscribed in the circle, and then divide the arc A F into three equal portions by the points G and Z.
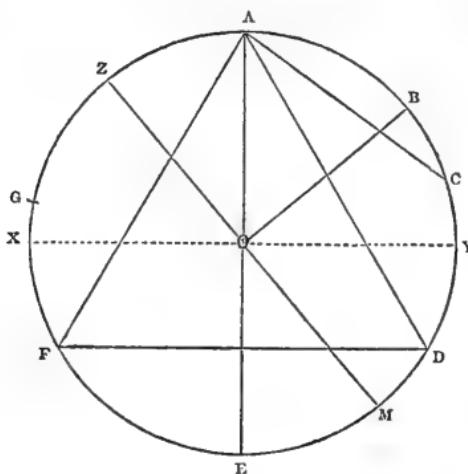
Regarding the whole circle as divided into 360 degrees, add to this amount successively the number of degrees in the arcs AB, BC, CD, DE, EF, FG, and GA, these arcs measuring respectively 45, 27, 48, 60, 60, 40, and 80 degrees, thus producing the series 360, 405, 432, 480, 540, 600, 640, 720. Then divide this series by the last term, and we obtain the following: 0·5, 0·5625, 0·6, 0·66', 0·75, 0·833', 0·88', and 1, a series from which the ratios may be deduced in reference to the longest wave as 1.

But it is more convenient so to alter the series as to make 1 = the mean wave. For this purpose draw the diameter z M,

and to 360° add 200, being the number of degrees in the arc
A M, so making 560; then dividing, as before, by 720, there
results 0·77′ as the fraction corresponding to the mean wave in
the foregoing series, which must accordingly be divided by this
fraction. The quotients, taken in their reverse order, stand
thus: 1·2857, 1·1428, 1·0714, 0·9643, 0·8571, 0·7714, 0·7232,
0·6429, the mean wave being =1.

The relative wave-lengths, however, as determined by Newton,
are represented, not by these numbers themselves, but by the
cube roots of their squares. The series then becomes 1·1824,
1·0931, 1·0470, 0·9761, 0·9023, 0·8411, 0·8057, 0·7449, the
length of the mean wave being =1. The ascertained length of
this last in air is 0·00002247 decimal parts of an English inch;
and the lengths of the others are found by multiplying by this
quantity the above numbers; so producing the following series,
which represents, according to Newton's estimate, the extreme
wave-lengths between which the different tints of the solar spec-
trum lie, in decimal parts of an English inch :—

Red	0·00002657
Orange	0·00002456
Yellow	0·00002353
Green	0·00002193
Blue	0·00002028
Indigo	0·00001890
Violet	0·00001812
	0·00001674

This accordingly is the estimate given of these wave-lengths
in all the English works on physical optics.

The next step in this inquiry was made by Fraunhofer, when
he discovered the fixed lines of the solar spectrum, and deter-
mined the wave-lengths corresponding to the points occupied by
the seven principal lines, which he designated by the letters
B, C, D, E, F, G, H. His observations are understood to have
been made with most careful accuracy; and he left on record
two sets of values of these wave-lengths, agreeing as respects B
and D, but differing slightly as respects the other five. These
two sets of values, stated in decimal parts of a *French* inch, the
standard employed by Fraunhofer, are as follow :—

I. B 0·00002541, C 0·00002425, D 0·00002175, E 0·00001943, F 0·00001789,
II. B 0·00002541, C 0·00002422, D 0·00002175, E 0·00001945, F 0·00001794,
                +3   −2   −5

    I. G 0·00001585, H 0·00001451
    II. G 0·00001587, H 0·00001464
          −2   −13

No attempt has yet been successfully made to reduce these

numbers to any determinate law. The only relation hitherto recognized is one subsisting in the *second* set, in which it is found that, if the *reciprocals* of the numbers be compared, the square of that of F is equal to half the sum of the squares of those of B and H, or $F_r^2 = \frac{1}{2}(B_r^2 + H_r^2)$, the corresponding numbers being

$$F_r^2 \quad . \quad . \quad . \quad = 3107100$$
$$\tfrac{1}{2}(B_r^2 + H_r^2) \quad . \quad = 3107068$$

Difference . $\overline{0000032}$

In the first set this relation does not subsist with nearly the same degree of accuracy.

There is, however, a relation of more importance which may be established by a very slight alteration upon the values of B and D. Although the observed values of these two agree in both series, neither of them can be regarded as correct to within a ten thousandth part of its magnitude. Now by altering each to a smaller amount than this proportion, we may establish the remarkable relation $B^5 = D^6$. Thus the observed value of B^5 (taking seven effective numbers) is 1594052

And that of D^6 is 1593953

Difference only . $\overline{0000099}$

Dividing this difference, we make each 1594025, which gives $B = 2540844$ (log 4049780), differing from the observed value by only ·0000156, and $D = 2175112$ (log 3374816), differing from the observed value by only ·0000112. As these alterations are so trifling, and this relation of $B^5 = D^6$ is very convenient, there need be no hesitation in adopting it, and regarding the above as the settled values of B and D.

There is a similar close approximation by which the value of the wave-length of E may be deduced from those of B and D. It is $B^7D = E^{11}$. Thus—

The observed value of E in the first series is .	1943000
The corrected log B 4049780 × 7 =	2·8348460
Add corrected log D	·3374816
Divide by 11	3·1723276
Gives for log E	0·2883934 = 1942645
Difference only	·0000355

This difference being much within the limits of probable errors of observation, the above may be regarded as a convenient relation by which to connect the value of the wave-length of E with those of B and D, and the true logarithm of E may accordingly be assumed as 0·2883934. The nearer approach to this value exhibited by the first observed series may be viewed as one ad-

vantage possessed by it over the second, which makes the value of E 1945.

Another advantage presented by the first series will be brought to light by the following arrangement:—Let the whole of the wave-lengths be formed into an equicentral series of fractions, thus, $\dfrac{B}{H}, \dfrac{C}{G}, \dfrac{D}{F}, \dfrac{B}{E}, \dfrac{C}{E}, \dfrac{D}{E}, \dfrac{E}{F}, \dfrac{E}{G}, \dfrac{E}{H}$, in which each greater is divided by each less, regarding E as the centre of the system. Arranging the quotients in the order of their magnitudes, call $\dfrac{B}{H} = 0$, $\dfrac{C}{G} = \pi$, $\dfrac{E}{H} = \rho$, $\dfrac{B}{E} = \sigma$, $\dfrac{C}{E} = \tau$, $\dfrac{E}{G} = \upsilon$, $\dfrac{D}{F} = \phi$, $\dfrac{D}{E} = \chi$, and $\dfrac{E}{F} = \psi$: the following are the values of these quantities according to the two sets of observations, adopting in both the above adjusted values of B and D, and in the case of the first series, the above adjusted value of E.

	1st set.	2nd set.	Differences.
$o =$	1·751099	1·735549	0·015550
$\pi =$	1·529968	1·526150	0·003818
$\rho =$	1·338832	1·328562	0·010270
$\sigma =$	1·307930	1·306347	0·001583
$\tau =$	1·248298	1·245244	0·003054
$\upsilon =$	1·225643	1·225583	0·000060
$\phi =$	1·215826	1·212437	0·003389
$\chi =$	1·119665	1·118310	0·001355
$\psi =$	1·085883	1·084169	0·001714

In this series we have $\rho = \dfrac{o}{\sigma}$, $\upsilon = \dfrac{\pi}{\tau}$, and $\chi = \dfrac{\phi}{\psi}$, so that only six of the nine members are primary.

Now if in both series the differences between the terms o, π, σ, and ψ be taken, they will stand thus:—

	Nos.	Diff.		Nos.	Diff.
1st Ser. $o =$	1·751099	0·221131	2nd Ser. $o =$	1·735549	0·209399
$\pi =$	1·529968	0·222038	$\pi =$	1·526150	0·219803
$\sigma =$	1·307930	0·222047	$\sigma =$	1·306347	0·222178
$\psi =$	1·085883		$\psi =$	1·084169	

The near approach to a common difference of 0·222′ is here too striking to be overlooked, and too important to be thrown aside,—the more especially as, in the case of the first series, the alterations required to make this progression perfect are very slight—a second advantage which it enjoys over its rival. Further, if in each case we take the sum of the first and middle terms, or $o + \tau$, they will stand as under:—

1st Ser.	$o=1{\cdot}751099$		2nd Ser.	$o=1{\cdot}735549$
	$\tau=1{\cdot}248298$			$\tau=1{\cdot}245244$
	$\overline{2{\cdot}999397}$			$\overline{2{\cdot}980793}$

Thus in both cases the sum is very nearly 3, but considerably nearer in the first than in the second; and as $o+\tau=3$ is a very convenient relation, this may be viewed as a third advantage presented by the first series, which requires only a trifling alteration to make it fulfil this condition, as well as that of having o, π, σ, ψ in arithmetical progression. These advantages afforded by the first series overweigh the single advantage furnished by the second, of presenting the before-mentioned relation of $F_r^2 = \frac{1}{2}(B_r^2 + H_r^2)$, which is of little comparative value; whereas the relations presented by the first series, when perfected, afford the great facility of rendering the whole of the wave-lengths deducible from that of either B or D alone.

The first series, when properly adjusted to the several relations, $B^5 = D^6$, $B^7 D = E^{11}$, $o = \pi + 0{\cdot}222'$, $\pi = \sigma + 0{\cdot}222'$, $\sigma = \psi + 0{\cdot}222'$, and $o + \tau = 3$, will stand as under:—

	Logs.	Nos.	Differences.	
$o =$	$0{\cdot}2436268$	$1{\cdot}752374$		
$\pi =$	$0{\cdot}1847346$	$1{\cdot}530152$		$0{\cdot}222222'$
$\rho =$	$0{\cdot}1270422$	$1{\cdot}339807$	$0{\cdot}190345$	
$\sigma =$	$0{\cdot}1165846$	$1{\cdot}307930$	$0{\cdot}031877$	$0{\cdot}222222'$
$\tau =$	$0{\cdot}0960845$	$1{\cdot}247626$	$0{\cdot}060304$	
$\upsilon =$	$0{\cdot}0886501$	$1{\cdot}226451$	$0{\cdot}021175$	
$\phi =$	$0{\cdot}0848012$	$1{\cdot}215630$	$0{\cdot}010821$	$0{\cdot}222222'$
$\chi =$	$0{\cdot}0490882$	$1{\cdot}119665$	$0{\cdot}095965$	
$\psi =$	$0{\cdot}0357130$	$1{\cdot}085708$	$0{\cdot}033957$	$0{\cdot}666666'$

It will thus be perceived that, while the terms of the series are nine in number, they are divisible into three groups; that the sum of the first and middle term is 3; that each of the common differences is $\frac{2}{9}$ and their sum $\frac{2}{3}$,—relations sufficiently remarkable in themselves, and easily borne in mind.

The following are the values of the wave-lengths corresponding to the fixed lines as deduced from the above series, and as compared with the first set of observed values:—

	Calculated.	Observed.	Differences.	
B	2540844	2541000		-0000156
C	2423694	2425000		-0001306
D	2175112	2175000	$+0000112$	
E	1942645	1943000		-0000355
F	1789289	1789000	$+0000289$	
G	1583957	1585000		-0001043
H	1449944	1451000		-0001056

These differences are so far within the limits of probable errors of observation, being all of them less than the least of the differences between the corresponding members of the two observed series, that there need be no hesitation in admitting them for the sake of obtaining a series so regular as the foregoing, and presenting the peculiar advantage of rendering the whole of the wave-lengths deducible from that of either B or D.

In all calculations involving these wave-lengths, it will be found more convenient to adopt, instead of the actual lengths corresponding to any standard of mensuration, the *relative* wave-lengths referred to that of B as unity, stating the others in fractional parts; thus keeping the numbers independent of any standard of linear measure. The wave-lengths and their logarithms will then stand as under.

Relative wave-lengths referred to B as unity.

		Logarithms.	Numbers.
	C	$\overline{1}\cdot9794999$	0·9538934
	D	$\overline{1}\cdot9325036$	0·8560588
	E	$\overline{1}\cdot8834154$	0·7645667
	F	$\overline{1}\cdot8477024$	0·7042103
	G	$\overline{1}\cdot7947653$	0·6233979
	H	$\overline{1}\cdot7563732$	0·5706545
Mean wave	M	$\overline{1}\cdot9701116$	0·9334940

It remains to compare the values of the wave-lengths corresponding to the fixed lines, with those found by Newton's series for the boundary lines of the coloured spaces of the solar spectrum; and for this purpose the latter must be reduced to the standard of the *French* inch, when they will be found to stand as under :—

	Newton's wave-lengths.	Fixed lines mean ob.
Red . . .	0·000024929	B 0·000025410
Orange .	0·000023046	C 0·000024235
Yellow .	0·000022074	
Green . .	0·000020579	D 0·000021750
Blue . .	0·000019023	E 0·000019440
Indigo .	0·000017733	F 0·000017915
Violet . .	0·000016987	G 0·000015860
:	0·000015705	H 0·000014575

This Table shows that the Newtonian values are not reconcileable with those of Fraunhofer; because they make the line B lie beyond the red, and the line H beyond the violet end of the spectrum. The cause of this discrepancy is traceable to Newton's having made his observations on an impure spectrum; Fraunhofer having been the first to obtain the pure spectrum, produced by numerous fine equidistant lines, and from which his wave-lengths were determined.

It is remarkable, however, that if we adopt Newton's *primary* series without subjecting it, as he did, to the operation of taking the cube roots of the squares, we shall obtain from it values for the wave-lengths corresponding to the boundary lines of the seven prismatic colours, agreeing much better with Fraunhofer's values for the wave-lengths corresponding to the fixed lines.

This series, reduced to the standard of the *French* inch, stands as under:—

	Borders of colours.		Fixed lines.	
Red. . .	0·000027107		B	0·000025410
Orange .	0·000024094		C	0·000024235
Yellow .	0·000022588	Mean wave	M	0·000023719
Green . .	0·000020330		D	0·000021750
			E	0·000019440
Blue . .	0·000018070		F	0·000017915
Indigo .	0·000016264		G	0·000015860
Violet. .	0·000015247		H	0·000014575
	0·000013554*			

In this Table, not only are the fixed lines brought within the spectrum, but each is referred to nearly its proper position. It were desirable that fresh observations be made on the wavelengths corresponding to the border lines of the colours in the *pure* spectrum, to ascertain whether these are accurately represented by the above series, or whether some other must be found which shall more correctly exhibit their ruling law. The subject is worthy of the attention of the British Association, were it only to prevent the existing error, in regard to the estimated value of these wave-lengths, from being any longer perpetuated.

* This series makes the interval between the extreme violet and the extreme red as 1 to 2, corresponding to the musical octave.

LX. *On the Thickness of the Crust of the Earth.* *By the* Rev.
SAMUEL HAUGHTON, *F.R.S., Fellow of Trinity College, and
Professor of Geology in the University of Dublin.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN the April Number of the Philosophical Magazine, Arch-
deacon Pratt replies to my communication published in
December, 1859.

So far as the controversy is personal between us, it turns on
a very simple mathematical question, which I am quite willing
to leave for the decision of mathematicians. I regret that my
statements on this subject have not been sufficiently clear, though
I endeavoured to make them so, and must therefore beg the
favour of a few lines on the matter, before entering on the
other question raised by the present controversy.

I. *The supposed "fallacy" in my reasoning.*

For the convenience of reference, I shall call the two equations
in dispute (A) and (B). Archdeacon Pratt makes the following
statements :—

May, 1859.	April, 1860.
"*Equation* (B) *does not follow from equation* (A) *by differentiation.* In fact equation (B) assumes that the law of density and ellipticity is continuous throughout the whole mass, solid and fluid, the solid parts lying in strata of the form and density they would have if they were wholly fluid."	" Professor Haughton . . . . re- plies to my reasoning by showing that he has differentiated equation (A) right. *This I never called in question.*"

Archdeacon Pratt now admits that equation (B) may be ob-
tained from equation (A) by differentiation, but he has omitted
to see that I expressly state that equation (B) can only be ap-
plied to the *fluid* nucleus of the earth, and that I so apply it in
order to diminish by one, the total number of unknown quantities,
which must become known before the thickness of the earth's
crust can be determined. My words are,—

[Equation B] "determines the relation which necessarily exists
between the law of density and ellipticity of the *fluid* portions
of the earth*."

To prevent further misconception, I shall here briefly repro-
duce my argument, intended to show that our speculations on
the thickness of the earth's crust, if it have a crust at all, are
essentially hypothetical.

If the earth have a solid crust, containing a liquid nucleus

* Transactions of the Royal Irish Academy, vol. xxii. Science, p. 265.

inside it, its outer surface, as proved by observation, and its inner surface, being the first fluid layer, *ex necessitate rei*, are perpendicular to the force of gravity. The general condition requisite for any surface of given specific gravity of the earth to be perpendicular to gravity, is contained in equation (A), which I here reproduce.

$$0 = \frac{e}{a}\int_0^a \rho a^2 - \frac{1}{5a^3}\int_0^a \rho \frac{d.a^5 e}{da} - \frac{a^2}{5}\int_a^a \rho \frac{de}{da} - \frac{ma^2}{2a^3}\int_0^a \rho a^2. \quad . \quad \text{(A)}$$

In this equation, the letters signify,—

e, the ellipticity of any layer;

a, the equicapacious radius of that layer;

ρ, the specific gravity of the layer;

a, the equicapacious radius of the outer surface of the supposed crust;

m, the ratio of centrifugal force to gravity at the equator.

This equation (A) applies to the outer surface of the crust, and to the outer surface of the fluid nucleus, both of which are perpendicular to gravity.

Let a, ϵ denote the radius and ellipticity of the outer surface of the crust;

and let a_1, ϵ_1 denote the radius and ellipticity of the outer surface of the supposed fluid nucleus: then equation (A), applied to these two surfaces, will become

$$\frac{2}{5}\int_0^a \rho \frac{d.a^5 e}{da} = (2\epsilon - m)a^2 \int_0^a \rho a^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(A}_1\text{)}$$

and

$$\frac{\epsilon_1}{a_1}\int_0^{a_1} \rho a^2 - \frac{1}{5a_1^3}\int_0^{a_1} \rho \frac{d.a^5 e}{da} - \frac{a_1^2}{5}\int_{a_1}^a \rho \frac{de}{da} = \frac{ma_1^2}{2a^3}\int_0^a \rho a^2. \quad . \quad \text{(A}_2\text{)}$$

The first of these equations (A$_1$) gives Clairaut's theorem, but teaches us absolutely nothing of the structure of the interior of the earth, except that it must be arranged in nearly spherical strata, each of constant density, or in some way or other equivalent to this.

The second equation (A$_2$) contains four definite integrals; viz.

$$\text{I.} \quad \int_0^a \rho a^2.$$

This integral extends through the whole earth, and is known, because the *mass* of the earth is known.

$$\text{II.} \quad \int_0^{a_1} \rho a^2, \text{ and } \int_0^{a_1} \rho \frac{d.a^5 e}{da}.$$

These integrals extend through the fluid nucleus, and are un-

known, because they depend on its mass, and moments of inertia. If we assume the *law of density*, they will both become known, or at least capable of evaluation, because the ellipticity is a function of the density, by virtue of equation (B), which belongs to the fluid nucleus, and to it only.

$$\text{III.} \quad \int_{a_1}^{a} \rho \frac{de}{da}.$$

This integral extends through the crust, and is unknown. It can only become known by our being acquainted with the *law of density*, and also the *law of ellipticity* of the layers of the crust, which are not connected with each other by an equation, as is the case in the fluid portion of the earth.

If these preliminary difficulties were overcome, and the values of the definite integrals known in terms of a_1 and known numbers, since ϵ_1 is also a function of a_1 (because it is included in equation (B) as part of the fluid nucleus), the equation (A_2) would become simply a function of a_1 and this unknown quantity, the radius of the fluid nucleus, might be easily found.

The hypotheses requisite are the following:—

1st. The law of density of the fluid portions of the earth.

2nd. The law of density of the solid portions.

3rd. The law of ellipticity of the solid portions.

Of these three essential laws, I maintain that we are in ignorance, and must be content to remain so; and I challenge Archdeacon Pratt, or any other person possessed of "positive" knowledge of the interior of the earth, to state what these laws are. I am, indeed, well aware that a chance guess of Laplace's as to the first law, has been considered by some almost an established law of nature, and I would therefore offer a few observations upon it, to show how improbable it is that it should be even an approximation to the real law of density that prevailed in the layers of earth when altogether fluid, or in the layers of it that are still fluid, if there be any such.

Legendre first applied the following law of density to the determination of the earth's figure,

$$\rho = \frac{A}{a} \sin na,$$

where—

$\rho =$ density of any layer,

$a =$ the equicapacious radius, and

A, n are constants to be determined.

Laplace knew well what the meaning of this law was; for in discussing it in the Eleventh Book of the *Mécanique Céleste*, he says, " Je vais présentement considérer la figure de la terre, en la supposant formée d'un *seul fluide compressible* " (*Méc. Cél.*

tom. v. p. 48). Mr. Hopkins and Archdeacon Pratt have adopted this law, although, as it appears to me, it is utterly inconsistent with the little we do know of the interior of the globe.

It is in the highest degree probable that the specific gravities of the successive layers of the globe depend almost altogether on their chemical composition, which is very varied, and only in a very slight degree on the pressure to which they are subject; and that, consequently, a theory like that of Laplace, which supposes the chemical composition uniform, and the density to depend on an assumed law of compressibility, must be rejected, as a matter of course, by every mathematician who wishes to have a positive basis of fact for his speculations. For this reason, I believe the charge which Archdeacon Pratt has brought against me, of having attempted " an algebraical, not a physical problem of densities," might with more fairness be brought against his own unauthorized assumption of Laplace's law, which he considers " in itself a very probable law."

From a consideration of the igneous rocks of various ages of the crust of the earth, many geologists have come to the conclusion that the two outer layers of that crust are composed of siliceo-felspathic rocks and ferro-calciferous eruptive rocks, having average specific gravities of 2·55 and 3·00. The difference in specific gravity of these layers is evidently due to the presence of iron in the latter, and has no relation whatever to the pressure to which they have been subjected. Such facts as this are completely ignored by the *merely mathematical* assumption that the whole earth is composed of a homogeneous mass of fluid following a supposed law of compressibility. I believe, therefore, that I am entitled to deny, as a matter of fact, that we possess any positive knowledge of the interior of the earth; and I shall retain my conviction that such knowledge is beyond our reach, until it is acquired by some process more legitimate than unfounded hypotheses, which are contradicted by the few facts that actually do come under our observation.

Before leaving this subject, it is worth while observing that Archdeacon Pratt's logic is as peculiar as his mathematics; for while he supposes that he has disposed of my sceptical argument by the detection of a supposed fallacy in my mathematics, he omits to perceive that, if I were really guilty of the fallacy, it would strengthen my argument against our positive knowledge of the interior of the globe, as it is plain that if I am not entitled to use the equation (B) to establish a relation between the ellipticity and density of the fluid portion of the globe, I must make an additional hypothesis, and therefore be forced to discuss equations involving *four* unknown quantities instead of *three*.

II. *Archdeacon Pratt's demonstration that the crust of the earth cannot be very thin.*

Before discussing this question, I would premise that I am not an advocate for the idea, held by many physical geologists, that the earth has a crust, and that its crust is very thin. I believe it to be as unphilosophical to maintain it to be thin, as to hold it to be thick, and that no good reason can be given for either opinion.

The following idea of the interior of the earth is one which I entertain myself with, but which I have no right to force upon another, viz. that the earth is composed of three layers—of granite, basalt or diorite, and meteoric iron and nickel, with an immense cavity in the centre caused by centrifugal force—and that it is completely solid at present. This, however, is a speculation, as unfounded as any of those I have attacked; and I must return to the demonstration that the earth's crust cannot be very thin.

This demonstration consists of three parts:—

1. If very thin, the mountains would fall through.

2. If very thin, the floor of the oceans would be forced up.

3. If very thin, a semi-diurnal fracture would be caused by the tides of the fluid nucleus.

I have already shown that Archdeacon Pratt's mode of considering the first of these questions is mechanically erroneous, as he supposes the mountain mass to be in a state of tension, whereas it is in a state of compression, and supports itself on the principle of the arch.

In the second case, although the floor of the ocean is in a state of tension, if the voussoirs be supposed to coincide with the radii of the earth, yet we know so little of the real direction of the main joints, that it is unsafe to speculate about them, although their fan-shaped arrangement under the mountain axes would appear to indicate a provision to sustain the weight, or rather is itself a consequence of the superincumbent weight.

I prefer, however, to deny the validity of Archdeacon Pratt's proof on the following grounds. As we know nothing of the interior of the earth, I am as well entitled as any other person to make hypotheses, and I accordingly make the following:—

1. The mountain chains float, like icebergs, on the surface of the fluid nucleus, having deep roots penetrating far down into the denser fluid below.

2. The liquid displaced by the roots of the mountains, finds lodgement in cavities scooped out under the floors of the deep oceans, thus restoring the hydrostatical equilibrium of the crust, which is thinnest under the oceans, and thickest under the mountains.

3. The effect of the tide caused by the sun and moon is rendered insensible at the surface by the great viscosity of the liquid at the bounding surface, which can only be called a fluid by courtesy. This viscosity distributes and destroys the pressure.

Let the foregoing hypotheses be assumed, which are quite as likely as any hitherto adduced, and it follows easily that the earth's crust, if it have one at all, need not exceed ten miles in thickness.

In conclusion, I would observe that in this controversy I have a natural advantage of position, which I am not prepared to relinquish. I deny our knowledge of the interior of the globe; on this subject I maintain that our ignorance is absolute and necessary. If Archdeacon Pratt possesses peculiar sources of information on this subject, let him give us the benefit of his knowledge; but he may rest assured that something more is necessary than reiterated assertion, and that to accuse an opponent of a fallacy which has no existence but in his own misconception of a mathematical principle, neither convinces others, nor advances his own cause.

<div style="text-align:center">I am, yours sincerely,
SAMUEL HAUGHTON.</div>

Trinity College, Dublin,
May 8, 1860.

LXI. *On a new Theoretical Determination of the Velocity of Sound. By the* Rev. SAMUEL EARNSHAW, *M.A., Sheffield.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I AM perfectly aware the problem of the propagation of sound is considered to have been solved; but notwithstanding this I venture to offer the following new solution to the notice of the philosophic world; because it not only leads to a numerical result quite different from any before obtained from theory, and agreeing better with experiment, but likewise furnishes some new results of an unexpected character, and affords besides a glimpse into a department of nature which has hitherto remained hermetically sealed. Laplace's ingenious suggestion of a change of temperature due to a sound-wave, brought the result of theory so very near to that of experiment, that it has been thought unreasonable to require a closer agreement. But it is confessed that the experiment by which the effect of a change of temperature is obtained is one that is remarkably difficult to manage, —one also in which errors of observation are greatly magnified in the result: this is shown to be so, from the great differences between the results of different experimentalists; and I think I may say that the requisite value of the coefficient (commonly

denoted by k) is much greater than Dalton's experiments warrant, and than what would have been conjectured *à priori* to be its value. In looking also at the determinations of its value, and also of the value of the velocity of sound, I am a little suspicious that modern experimentalists have suffered themselves to be biassed by a desire to make experiment and theory agree. At any rate, if we compare experiments made since 1816, when Laplace announced his theorem for the correction of Newton's result, with those previously made, it is impossible not to notice a very sudden and startling change ; and in the same spirit the value of k has been gradually growing in the hands of experimentalists till it is now large enough really to justify the opinion which has been expressed, that to Laplace is due the honour of having completed the solution, which was begun in England, of the problem of the propagation of sound. And, to speak candidly, it must be confessed that Laplace's sagacious suggestion undoubtedly has the air of a *vera causa*, although it requires a larger development of heat by the sound-wave than seems probable. But its great defect, if I may be allowed to consider it defective, is that the result it gives does not come up to experiment. The theoretical velocity, after being amended by Laplace's suggestion, still falls short of the experimental velocity by 24 feet, if we take this last to be 1090 feet ; and by 76 feet, if we take the velocity of sound to be 1142 feet as determined by Derham, Flamsteed, Halley, and the Florentine Academicians. It should be remembered also that theory might *à priori* be expected to give a result exceeding, rather than falling short of, experiment ; for theory assumes the elasticity and fluidity of the atmosphere to be perfect, and we have reason to think both are really in a slight degree imperfect ; and this is not likely to accelerate, but rather to retard (if it at all affect) the propagation of sound-waves. Upon the whole, after considering the matter in as impartial a spirit as possible, candour obliges me to confess that Laplace's suggestion does not furnish a *sufficient* cause. I do not deny that it may be a cause ; but it is not the whole. There is a cause, still unrevealed, for the defect of the theoretical velocity. Euler considered that some part of the error of theory might be due to the incorrectness of analysis in assuming $\left(\dfrac{dy}{dx}\right)^2 = 1$ previously to integrating the differential equation ; and certainly, as this was an arbitrary step, it was reasonable to suppose it might in some way have the effect of making the theoretical result smaller than it would be were the equation integrated without making use of approximative steps. When therefore I succeeded in integrating it without approximative steps, I was disappointed to find that the theoretical velocity of a sound-

wave remained the same as before. This caused me to examine the question of the propagation of waves in an elastic medium *ab initio*; and the result is that I have, I believe, detected a flaw in the problem as previously treated, which being remedied, there results from theory a value of the velocity of sound which agrees *accurately* with the experimental value found by Pictet, and with the following remark of Young:—" From a comparison of the accurate experiments of Derham, made in the daytime, with those of the French Academicians, made chiefly at night, it appears that the *true velocity of sound is about* 1130 *feet in a second;*" and this agrees exactly with the value which I obtain by the theory, which I will now proceed to lay before your readers.

1. There is a fundamental difference in the mechanical actions of two elastic media, one of which is supposed to be continuous, and the other to consist of particles separated by finite intervals. Let A Z be an elastic medium; divide it by imaginary planes into extremely thin slices D, E, F, . . . , so thin that each contains only one layer of particles; in other words, the thickness of the slices will be equal to the distance between the particles of the medium. Now according to the common solution of the problem of sound, the medium is supposed to be continuous; that is, any slice (as F) is pressed upon only by the two slices (E, G) with which it is in immediate contact. And, corresponding to this, any slice (F) is supposed to exert no direct pressure on any slices beyond the two (E, G) with which it is in contact. Hence *all* the motion which any one slice (F) has, it received from one of its immediate neighbours (E), and transmits it wholly to the other (G). This is the system of medial action supposed in the investigation of the differential equation of sound as commonly given.

But this supposes molecular action to extend from any one particle to those only which are nearest to it,—a supposition for which there is no foundation whatever in nature. It is certain, however, that molecular action extends to a very small finite distance, and therefore enables molecules which are separated by any distance not exceeding that, to act on each other. Hence any slice (F) is pressed upon by H, I, . . . as well as by G, on one side; and by D, C, . . . as well as E, on the other; and not *all*, but only *some portion of,* the motion which (F) receives does it receive from E; the rest comes from D, C, ...; and the motion which F has received it does not wholly transmit to G, but it distributes it among G, H, I, . . . the slices within reach of its

molecular action. This is the system of medial action supposed in the investigation which I have to produce.

2. It is to be noticed, also, that when the medium is supposed to be continuous, the *whole* force exerted on F on one side is supposed to be exerted by the slice E, and on the other by the slice G; but on the other supposition, these forces are distributed among the slices E, D, C ... on one side, and G, H, I ... on the other, according to some rapidly decreasing law, which we shall have to determine. Consequently the force which on the first supposition is exerted by one slice (E) upon F, is on the latter hypothesis exerted by E, D, C ... unitedly; so that the force in the former case exerted by E alone is equal to the *sum* of the forces exerted by E, D, C ... in the second case.

3. Let z be the distance of any slice D from F, and let H be at the same distance on the other side of F. Then we may represent by $mf(z)$ the force exerted by either of the slices D, H on a particle in F. Hitherto we have supposed the medium in equilibrium, let it now be in a state of *motion*; and for simplicity let us suppose all the particles in any slice to be in the same state of disturbance. Denote by x_{i}, x, x^{i} the disturbances of the slices D, F, H at the time t. Then the whole force exerted by D and H on a particle of the slice F

$$= mf(z+x-x_{i}) - mf(z+x^{i}-x) = -mf'(z) \cdot \{x_{i}-2x+x^{i}\},$$

neglecting powers of $x-x_{i}$ and $a^{i}-x$ above the first. Now this step supposes that the *relative* displacements of any two particles which are within the sphere of each other's action, is so small in comparison of their distance from each other, that the square and higher powers of it may be neglected. The *absolute* displacements may be of any magnitudes, subject to this condition. Our results will therefore not be limited to small *absolute* motions, but to small *relative* motions of particles within the sphere of each other's action. Let now h be the thickness of the slices; and denote the disturbances of ... D, E, F, G, H ... from their equilibrium positions by ...$x_{r-2}, x_{r-1}, x_{r}, x_{r+1}, x_{r+2}$... respectively. Then the equation of motion of any particle of the slice F will be

$$\begin{aligned} D_t^2 x_r = &mf'(h) \cdot (x_{r-1}-2x_r+x_{r+1}) \\ &+ mf'(2h) \cdot (x_{r-2}-2x_r+x_{r+2}) \\ &+ mf'(3h) \cdot (x_{r-3}-2x_r+x_{r+3}) \\ &+ \ldots \ldots \end{aligned}$$

4. It is not very difficult to exhibit symbolically the general integral of this equation; but that is no part of my present object, which is to find the velocity with which any disturbance is propagated through the medium. I shall therefore, for the

sake of simplicity, assume the disturbance to be of the type which satisfies the equation $x_r = A_r \cos(kt)$, A_r being a function of r but not of t. By substituting this in the preceding differential equation, we obtain

$$-k^2 A_r = mf'(h) \cdot (A_{r-1} - 2A_r + A_{r+1})$$
$$+ mf'(2h) \cdot (A_{r-2} - 2A_r + A_{r+2})$$
$$+ mf'(3h) \cdot (A_{r-3} - 2A_r + A_{r+3})$$
$$+ \ldots \ldots$$

This being a linear equation of partial differences, its solution will be of the form

$$A_r = C\alpha^{2r} + C'\alpha^{-2r}, \quad \ldots \ldots \ldots \quad (1)$$

the quantity α being such as to satisfy the equation

$$-k^2 = mf'(h) \cdot (\alpha - \alpha^{-1})^2 + mf'(2h) \cdot (\alpha^2 - \alpha^{-2})^2 + \ldots \quad (2)$$

These results may be exhibited in a more simple form by writing $\alpha - \alpha^{-1} = 2\sqrt{-1}\sin\theta$, which reduces them to

$$\tfrac{1}{4}k^2 = mf'(h) \cdot \sin^2\theta + mf'(2h)\sin^2 2\theta + mf'(3h) \cdot \sin^2 3\theta + \ldots \quad (3)$$

and

$$A_r = 2A\cos(2r\theta + a); \quad \ldots \ldots \ldots \quad (4)$$
$$\therefore \ x_r = 2A\cos(2r\theta + a)\cos(kt)$$
$$= A\cos(kt - 2r\theta - a) + A\cos(kt + 2r\theta + a).$$

This is the general result for the type of wave which we have assumed; and it indicates that there may be two waves of that type travelling in opposite directions. For our purpose it will be sufficient to preserve one of them. Hence we have

$$x_r = A\cos(kt - 2r\theta). \quad \ldots \ldots \quad (5)$$

From this equation it results that if λ be the length of a wave, and v the velocity of its transmission,

$$\theta = \frac{\pi h}{\lambda}, \quad \ldots \ldots \ldots \quad (6)$$

and

$$v = \frac{k\lambda}{2\pi} = h\sqrt{m} \cdot \left\{ 1^2 \cdot f'(h) \cdot \left(\frac{\sin\theta}{\theta}\right)^2 + 2^2 \cdot f'(2h) \cdot \left(\frac{\sin 2\theta}{2\theta}\right)^2 \right.$$
$$\left. + 3^2 \cdot f'(3h) \cdot \left(\frac{\sin 3\theta}{3\theta}\right)^2 + \ldots \right\}^{\frac{1}{2}}. \quad \ldots \ldots \quad (7)$$

5. Now in the case of all sounds which are audible to human ears, λ is immensely larger than h; and consequently for all audible sounds, $\frac{\sin\theta}{\theta} = 1$, $\frac{\sin 2\theta}{2\theta} = 1$, &c.; and hence the velocity of transmission of sounds of every pitch, audible to our organs of hearing, though *not absolutely* the same, is *sensibly* the

same, and equal to

$$h \sqrt{m} \cdot \{1^2 \cdot f'(h) + 2^2 \cdot f'(2h) + 3^2 \cdot f'(3h) + \dots \}^{\frac{1}{2}}.$$

From this formula we learn that every slice produces a term in the expression for the velocity, so that there are as many terms in the expression for the velocity as there are slices within the radius of the sphere of action of any one particle.

6. From arts. 2 and 3 it is evident that the whole force exerted upon a particle of the slice F by all the slices on one side of F is $mf(h) + mf(2h) + mf(3h) + \dots$; and this is therefore the force which we must suppose concentrated in the slice E, and an equal force in the slice G, on the hypothesis of continuity. Hence if

$$m\mathrm{F}(h) = mf(h) + mf(2h) + mf(3h) + \dots,$$

then

$$m\mathrm{F}'(h) = mf'(h) + mf'(2h) + mf'(3h) + \dots;$$

and the equation of motion will be, for the case of continuity,

$$\mathrm{D}_t^2 x_r = m\mathrm{F}'(h) \cdot (x_{r-1} - 2x_r + x_{r+1}).$$

From this we obtain, as before, the expression for the velocity of transmission,

$$\text{vel.} = h \sqrt{m \cdot \mathrm{F}'(h)}.$$

But in this case we know the velocity of transmission is $\sqrt{\mu}$, the velocity determined by Newton,

$$\therefore \ \sqrt{\mu} = h \sqrt{m \cdot \mathrm{F}'(h)}$$
$$= h \sqrt{m} \cdot \{f'(h) + f'(2h) + f'(3h) + \dots \}^{\frac{1}{2}}.$$

7. Eliminating $\sqrt{m}$ between this equation and that of art. 5, there results, finally,

$$v = \sqrt{\mu} \cdot \left\{ \frac{1^2 \cdot f'(h) + 2^2 \cdot f'(2h) + 3^2 \cdot f'(3h) + \dots}{f'(h) + \ \ f'(2h) + \ \ f'(3h) + \dots} \right\}^{\frac{1}{2}}.$$

Now the numerator of this fraction is of necessity larger than the denominator; and therefore, on the very face of it, this expression indicates that the actual velocity of sound is greater than was found by Newton. It remains to determine the value of this expression.

8. We assume that $f(z)$, and therefore also $f'(z)$, is some simple inverse power of z. That power in the case of $f'(z)$ must be greater than 3; for if $f'(z)$ be equal to $\dfrac{\mathrm{C}}{z^3}$, then the expression in art. 5 gives the velocity of transmission

$$= h \sqrt{m} \cdot \left\{ \frac{\mathrm{C}}{h^3} + \frac{\mathrm{C}}{2h^3} + \frac{\mathrm{C}}{3h^3} + \dots \right\}^{\frac{1}{2}},$$

which is known to be infinite. The lowest possible value of the

power is therefore 4. Assuming, therefore, that $f'(z) = \dfrac{C}{z^4}$, we find the velocity of the transmission of sound

$$= \sqrt{\mu} \cdot \left\{ \frac{1 + \frac{1}{2^2} + \frac{1}{3^2} + \frac{1}{4^2} + \ldots}{1 + \frac{1}{2^4} + \frac{1}{3^4} + \frac{1}{4^4} + \ldots} \right\}^{\frac{1}{2}}$$

$$= \frac{\sqrt{15\mu}}{\pi}.$$

This put into numbers, taking $\sqrt{\mu}$ to be equal to 916 feet, gives the velocity of sound equal to 1130 feet.

Thus we see that the error committed in calculating the velocity of sound, was not the leaving out the consideration of the development of heat, but the supposing the medium of air to be *continuous*. I am surprised to find the result so much affected by a circumstance which appears trifling,—and the more so, as the radius of the sphere of sensible molecular action is known to be, though finite, very small. The assumption of continuity is therefore by no means so allowable as we should be inclined *à priori* to suppose; and its effect on the motion of an elastic medium is very much greater than was to be expected.

Sheffield, May 9, 1860.

[To be continued.]

LXII. *On a new kind of Electric Current.* By G. Quincke[*].

UNDER the above title, the fifth Number of Poggendorff's *Annalen* for 1859 contains an article of considerable length, the leading points in which are contained in the following abstract.

When pure water flows through a porous body, an electrical current is elicited,—a fact established by the following experiments.

A plate of burnt clay is luted with sealing-wax between two glass tubes of 25 millims. diameter (fig. 1), whose ends are worked down smooth. A pair of platina wires are melted into the side of the two tubes, and plates of platina are riveted on to

Fig. 1.

those wires, the wires themselves being connected with the terminals of a sensitive multiplier furnished with astatic needles. The tubes A and B are made smaller at the ends, for the convenience of connecting them with other tubes. The apparatus is now filled with distilled water, care being taken that no air
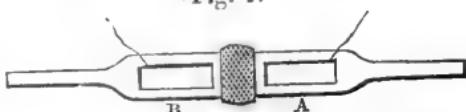
remains in the clay plate. If, then, either by suction at B or by some pressure at A, the fluid is driven from A to B through the clay plate, there is indicated, at the instant the flow of the fluid begins, a deflection of the needles, due to an electric current passing from A towards B. The platina plate B, then, on which the current strikes last, behaves like the platina plate of a Grove's element. As soon as the passing of the water ceases, the needle returns to its place, a polarization-current, however, in a contrary sense to the primary current and to the flow of the fluid, being called forth.

On changing the direction of the flow of the water, as by means of suction at A, the multiplier indicates an electrical current passing in the fluid from B towards A.

As with this form of the apparatus the flow of the fluid strikes the plates dissimilarly, thus causing a want of precise similarity in their conditions, the modification of the apparatus shown in fig. 2, by which the flow of the fluid is not directed against the platina plates at all, was arranged for the subsequent experiments.

With this view the tubes A, B of fig. 1 were closed at the end, and two narrower tubes, C, D of fig. 2, were adapted to the side between the open end of the tubes and the platina plates. With this arrangement the fluid against the plates remained unchanged, while the water flowed through C, the diaphragm, and D.

Fig. 2.

The pressure employed varied, according to the diaphragm used, from a third of an atmosphere to three atmospheres.

Instead of the clay plate, other porous bodies were placed between the tubes A and B of fig. 2 ; and the multiplier always indicated a current coinciding with the flow of the fluid, and which lasted as long as that flow did. On its ceasing, there was a more or less strong polarization-current in a contrary direction to the primary one.

The substances thus examined were,—

Silk,	Sulphur,
Linen,	Burnt clay,
Ivory,	Talc,
Glass,	Graphite,
Sand,	Bunsen's coal,
Fir-wood,	Iron,
Lime-wood,	Platina,
Oak,	

which were applied in the following way.
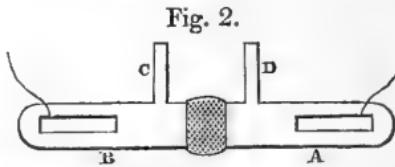
Some thirty layers of thin silk stuff were placed over each other and attached over the tube A of the apparatus; the tube B was then adapted against the former, and the part separating them covered thickly with sealing-wax.   Owing to the wide pores of the silk, considerably more water flowed through under equal pressure than when the clay plate was employed.   The linen was used in the same manner.

The other substances were applied in the form of powder, in a glass tube of the diameter of the tubes A and B of fig. 2.   The ends of these tubes, the length of which varied, according to the substance employed, from 20 to 45 millims., were ground flat, and over them were placed discs of the silk stuff spoken of, to prevent the flow of the fluid carrying away particles of the substance under examination.   In the case of Bunsen's coal the tube was closed with plates thereof.

Platina was made use of in the spongy form, iron as filings. The glass had been reduced to powder on an anvil.   Ivory and the various kinds of wood were employed in the form of sawdust. It was endeavoured in vain to press water through a porous plate of wood, for the plate had to be luted in dry; and on becoming moist, even if cut perpendicular to the direction of the fibres, it warped so much that it broke the sealing-wax or the tube.

The direction of the electric current was not changed by adding acids or solutions of salts to the distilled water, but it was considerably weakened thereby.

For instance, on using a new clay plate 3·9 millims. thick, and pressing distilled water through it, the needle of the multiplier was deflected up to the stop : on the addition of four drops of pure hydrochloric acid to a pint and three-quarters of the water, the deflection of the needle did not, under the same pressure as before, exceed 15 or 20 degrees.   A further addition of twelve drops of acid weakened the electric current so much, that a far greater pressure had to be applied to deflect the needle at all.   If the acid amounted to 16 per cent. of the fluid employed, no deflection whatever was observed, even under a pressure of three atmospheres.   On adding alcohol to the distilled water, the deflection of the needle was increased.

The question arises, what is it that causes these electric currents ?   If the tubes A and B of fig. 1 are luted together without any clay plate between them, and a stream of water is passed through them, no deflection is observable in the multiplier. Hence it is seen that the presence of a diaphragm is necessary for the manifestation of an electric current.

The law that obtains in all these experiments may be stated concisely in these terms :—

The electromotive force which is developed when a certain pressure forces pure water through a clay plate, is independent of the size and thickness of the plate, and also of the amount of water that has flowed through, but is proportional to the pressure employed.

The multipliers used for these experiments were such as are employed by M. E. du Bois-Reymond in his researches in organic electricity. In one of the instruments the wire was wound no less than 33,000 times round the frame, in the others 10,080 and 600 respectively.

In a subsequent Number of Poggendorff's *Annalen* (Part 11 for 1859), M. Quincke announces that he has since discovered that, by using flowers of sulphur as a porous diaphragm, the electromotive force, all other circumstances remaining equal, is incomparably greater; and that this substance is therefore better suited than burnt clay for forming a diaphragm-apparatus, so that now there will be no further difficulty in demonstrating these electrical currents under moderate pressures. The sulphur which was mentioned in the original paper as being used for a porous plate, was roll-sulphur ground to powder in an agate mortar, as were also the talc and the graphite. At the close of his supplementary notice, M. Quincke remarks that he has been able to establish the two following facts with respect to these electrical currents; first, that they produce chemical decomposition; and secondly, that they afford evidence of free electricity.

LXIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 398.]

Dec. 8, 1859.—Sir Benjamin C. Brodie, Bart., Pres., in the Chair.

THE following communications were read:—

"Supplement to a Paper 'On the Influence of White Light, of the different Coloured Rays, and of Darkness, on the Development, Growth, and Nutrition of Animals*. By Horace Dobell, M.D. &c.

The apparatus used in the following experiments, was described in my Paper; but in the present instance, only two of the cells were employed, viz. that exposed to ordinary white light, and that from which all light is excluded. In order more effectually to prevent the possible admission of light, the following precautions were adopted with the dark cell:—1. The perforated zinc floor was covered with thick brown paper. 2. The under surface of the lid was lined with black cloth, to secure accurate adjustment when shut. 3. The opaque black glass was covered with an additional coat of black oil-paint. 4. The lid was never opened in any light except that of a candle or of gas.

* Phil. Mag. S. 4. vol. xviii. p. 143.

March 20th, 1859.—A number of ova of the Silkworm (*Bombyx mori*), all of the same age, were placed in each of the two cells. No change was observed until *May* 18*th* (sixty days after the commencement of the experiments), when one larva emerged from the ovum in each cell; and during twelve days, larvæ continued to emerge in the light and in the dark at the same rate.

June 9th.—Sixteen larvæ, as nearly as possible of the same size, were selected in each cell, and the rest removed. The experiments then proceeded with these thirty-two individuals, and no death occurred from first to last.

The following Table shows the day on which each larva began to spin; the day on which the perfect insect escaped from the pupa; and hence the number of days occupied by the metamorphosis.

Light.			Darkness.		
Day of beginning to spin.	Day of escape of the Moth.	Number of days occupied by metamorphosis.	Day of beginning to spin.	Day of escape of the Moth.	Number of days occupied by metamorphosis.
July 1	July 18	18 days inclusive	June 30	July 18	19 days inclusive
,, 2	,, 19	18 ,, ,,	,, 30	,, 18	19 ,, ,,
,, 2	,, 19	18 ,, ,,	,, 30	,, 18	19 ,, ,,
,, 2	,, 18	17 ,, ,,	,, 30	,, 21	22 ,, ,,
,, 2	,, 18	17 ,, ,,	,, 30	,, 18	18 ,, ,,
,, 2	,, 19	18 ,, ,,	July 1	,, 18	18 ,, ,,
,, 2	,, 19	18 ,, ,,	,, 1	,, 18	18 ,, ,,
,, 3	,, 19	17 ,, ,,	,, 2	,, 18	17 ,, ,,
,, 3	,, 21	19 ,, ,,	,, 2	,, 19	18 ,, ,,
,, 4	,, 20	17 ,, ,,	,, 2	,, 20	19 ,, ,,
,, 4	,, 20	17 ,, ,,	,, 2	,, 19	18 ,, ,,
,, 4	,, 20	17 ,, ,,	,, 2	,, 20	19 ,, ,,
,, 4	,, 21	18 ,, ,,	,, 2	,, 21	20 ,, ,,
,, 4	,, 21	18 ,, ,,	,, 3	,, 21	19 ,, ,,
,, 5	,, 21	17 ,, ,,	,, 3	,, 20	18 ,, ,,
,, 6	,, 24	19 ,, ,,	,, 4	,, 21	18 ,, ,,

From this it is seen that the mean period occupied by the metamorphosis in the *darkened* cell was eighteen days fifteen hours, and in the *light* cell seventeen days sixteen hours.

The longest and shortest periods in the *darkened* cell twenty-two days and seventeen days, in the *light cell* nineteen days and seventeen days.

June 9th.—On selection of sixteen of the largest larvæ from the inhabitants of each cell, it was noted that, when sixteen were selected from the *darkened cell* and several of *similar size* removed, only four could be found as large in the *white cell*, the remaining twelve selected were therefore of a rather smaller size. This difference in the two cells became less obvious afterwards, but, throughout the experiments, there was a slight difference of size in favour of the darkened cell.

With these exceptions, no difference could be detected between the results obtained in the cell from which light was completely excluded and in that exposed to its full influence.

The larvæ, the silk produced, and the moths from the two cells,

when placed side by side, could not be distinguished from one another.

The ova were of the same colour when first deposited, and underwent the same changes of appearance, at the same time, in the dark and in the light.

So far, therefore, as the direct agency of light is concerned in the development, growth, nutrition, and coloration of animals, the results of these experiments closely correspond with those already recorded in my Paper.

"Supplement to a Paper 'On the Thermodynamic Theory of Steam-engines with dry Saturated Steam, and its application to practice.'" By W. J. Macquorn Rankine, C.E., F.R.S. &c.[*]

This supplement gives the dimensions, tonnage, indicated horse-power, speed, and consumption of fuel, of the steam-ships whose engines were the subjects of the experiments referred to in the original paper. Results are arrived at respecting the available heat of combustion of the coal employed, and the efficiency of the furnaces and boilers, of which the following is a summary :—

No. of experiment.	Kind of boiler.	Total heat of combustion of 1 lb. of coal in ft.-lbs., estimated from chemical composition.	Available heat of combustion of 1 lb. of coal in ft.-lbs., computed from efficiency of steam and weight of coal burned per I.H.P.	*Available heat,* total heat, = efficiency of furnace and boiler.
I.	⎱Improved Marine ⎰Boilers of ordi- ⎱nary proportions.⎰	10,000,000	5,420,000	0·542
III.		10,000,000	5,300,000	0·53
II.	⎧Boiler chiefly composed of small vertical water-tubes, with very great heating surface.⎫	11,560,000	10,110,000	0·88

Available Heat of Combustion of 1 lb. of coal

$$= \frac{1,980,000 \text{ ft.-lbs.}}{\text{Efficiency of steam} \times \text{lb. coal per I. H. P. per hour}}.$$

"Researches on the Phosphorus-Bases."—No. VII. Triphosphonium-compounds. By A. W. Hofmann, LL.D., F.R.S. &c.

In several previous communications I have submitted to the Royal Society the results which I have obtained in examining the deportment of triethylphosphine with dibromide of ethylene, as the prototype of diatomic bromides. I have shown that the final product of this reaction is a diatomic salt corresponding to two molecules of chloride of ammonium.

The further prosecution of the study of triethylphosphine in this direction has led me to investigate the derivatives generated by the

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[*] Phil. Trans. 1859, p. 177 ; and Phil. Mag. S. 4. vol. xviii. p. 71.

phosphorus-base when submitted to the action of triatomic chlorides, bromides, and iodides.

The most accessible terms of this group being chloroform, bromoform, and iodoform, the changes of triethylphosphine under the influence of these agents have more especially claimed my attention.

Action of Iodoform on Triethylphosphine.

Both substances unite with energy at the common temperature. In order to avoid the inflammation of the phosphorus-base, small quantities of the materials should be mixed at a time. The products of the reaction vary with the relative proportions of the two substances.

By adding gradually crystals of iodoform to a moderate bulk of triethylphosphine until a new addition produces no longer an elevation of temperature, a viscous mass of a clear yellow colour is obtained, which, when treated with alcohol, changes to a white powder of crystalline aspect; these crystals are easily soluble in water, difficultly soluble in alcohol, and insoluble in ether. Two or three crystallizations from boiling alcohol render them perfectly pure. The analysis of this body has led me to the formula

$$C_{38} H_{46} P_3 I_3,$$
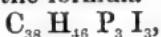
which represents a compound of one molecule of iodoform, and three molecules of triethylphosphine,

$$3 \, C_{12} H_{15} P + C_2 H I_3 = C_{38} H_{46} P_3 I_3.$$
$$\underset{\substack{\text{Triethyl-} \\ \text{phosphine.}}}{} \quad \underset{\text{Iodoform.}}{} \quad \underset{\text{New Compound.}}{}$$
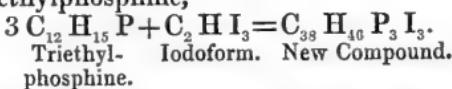
Iodoform thus fixes three molecules of triethylphosphine, giving rise to the formation of the tri-iodide of a triatomic metal, of a triphosphonium corresponding to three molecules of chloride of ammonium.

$$C_{38} H_{46} P_3 I_3 = \begin{bmatrix} (C_2 H)''' \\ (C_4 H_5)_3 \\ (C_4 H_5)_3 \\ (C_4 H_5)_3 \end{bmatrix} P 3 \end{bmatrix}''' I 3.$$
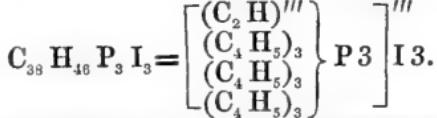
The aqueous solution of the iodide yields with iodide of zinc a white crystalline precipitate which is difficultly soluble in water, and appears to be slightly decomposed by recrystallization. It consists of one molecule of the triatomic iodide and three molecules of iodide of zinc,

$$C_{38} H_{46} P_3 I_3, 3 \, Zn I.$$

By treating the tri-iodide with the various salts of silver, a series of triatomic compounds is easily obtained, which contain the different acids.

The trichloride furnishes with dichloride of platinum a pale-yellow precipitate, which is insoluble in water, but dissolves in boiling concentrated hydrochloric acid. From this solution it is deposited on cooling in brilliant rectangular plates, which contain

$$C_{38} H_{46} P_3 Cl_3, 3 \, Pt Cl_2.$$

I have vainly tried to produce a trioxide which would correspond to the tri-iodide.

The tri-iodide is promptly attacked by oxide of silver, with formation of iodide of silver, and of an exceedingly caustic fixed base, which remains in solution. This base no longer belongs to the same series.

By treating its solution with hydriodic acid, or with hydrochloric acid and dichloride of platinum, it is at once perceived that the action of the oxide of silver has profoundly changed the original system of molecules. Hydriodic acid no longer produces the salt difficultly soluble in alcohol; by evaporating the solution a crystalline residue is obtained, which easily separates into a viscous, extremely soluble substance, and splendid crystals of an iodide, very soluble in water and alcohol, but insoluble in ether. The analysis of this iodide has proved it to contain

$$C_{14}\,H_{18}\,PI = \left[\begin{matrix}C_2\,H_3\\C_4\,H_5\\C_4\,H_5\\C_4\,H_5\end{matrix}\right\}\ P\ \right] I.$$
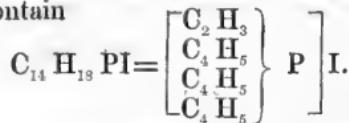
This formula represents the iodide of methyl-triethylphosphonium, which was formerly obtained by M. Cahours and myself, by acting with iodide of methyle upon triethylphosphine.

The alkaline liquid, obtained by the action of oxide of silver upon the tri-iodide, when saturated with hydrochloric acid, yields no longer the platinum salt, difficultly soluble in water but soluble in hydrochloric acid. In a dilute solution no precipitate whatever takes place, and only after considerable evaporation well-defined deep orange-yellow octahedrons are deposited, which contain

$$C_{14}\,H_{18}\,PCl,\ PtCl_2 = \left[\begin{matrix}C_2\,H_3\\C_4\,H_5\\C_4\,H_5\\C_4\,H_5\end{matrix}\right\}\ P\ \right] Cl,\ Pt\,Cl_2.$$
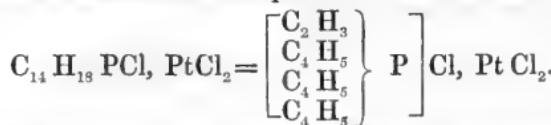
From these results it is obvious that the triphosphonium-salt, when submitted to the action of oxide of silver, passes over into a monophosphonium-compound. The latter is not the sole product of the reaction; I have already alluded to the viscous deliquescent substance which accompanies the iodide of methyl-triethylphosphonium. This is an iodide which, in the solution produced by the action of oxide of silver upon the original tri-iodide, exists in the form of oxide. The latter substance is easily recognized by evaporating the solution of oxide of methyl-triethylphosphonium, and adding a concentrated solution of potassa, when the oily globules characteristic of the dioxide of triethylphosphonium separate, which disappear immediately on addition of water.

The metamorphosis of the tri-iodide, under the influence of oxide of silver, is represented by the following equation:—

$$\left[\begin{matrix}(C_2\,H)'''\\(C_4\,H_5)_3\\(C_4\,H_5)_3\\(C_4\,H_5)_3\end{matrix}\right\}\ P_3\ \right] I_3 + 3\,AgO + 3\,HO = 3\,AgI + \left[\begin{matrix}C_2\,H_3\\C_4\,H_5\\C_4\,H_5\\C_4\,H_5\end{matrix}\right\}\ P\ \right\}_{H} O_2 + 2 \left(\begin{matrix}C_4\,H_5\\C_4\,H_5\end{matrix}\right\} PO_2\right)$$
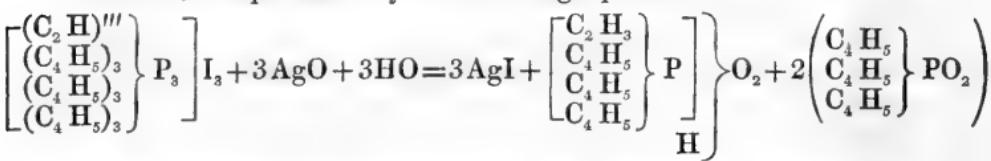
The tri-iodide which forms the subject of this Note is not the only product of the reaction between iodoform and triethylphosphine. There are other compounds formed, especially when the iodoform is employed in great excess. The nature of these bodies, which may be divined from the examination of the corresponding compounds in the diatomic series, is not yet fixed by experiment.

I have satisfied myself that chloroform and bromoform act like iodoform upon triethylphosphine.

The phosphorus-base acts, even at the common temperature, upon tribromide of allyle. The mixture of the two bodies solidifies into a crystalline mass, in the examination of which I am engaged.

The reactions which I have pointed out in this Note have induced me to extend my experiments to tetratomic bodies. The chloride of carbon, C_2Cl_4, obtained by the final substitution of chlorine for the hydrogen in marsh-gas, appeared to promise accessible results. On submitting this body, remarkable for its great indifference under ordinary circumstances, to the influence of triethylphosphine, I have observed with astonishment a most powerful reaction. Every drop of triethylphosphine which is poured into the chloride of carbon, hisses like water falling upon red-hot iron. On cooling, the mixture solidifies into a mass of white crystals, which will be the subject of a special communication.

December 15.—Sir Benjamin C. Brodie, Bart., Pres., in the Chair.

The following communication was read :—

"Comparison of some recently determined Refractive Indices with Theory." By the Rev. Baden Powell, M.A., F.R.S. &c.

In a series of papers inserted in the Philosophical Transactions (1835, 1836, 1837), and afterwards, in a more correct and complete form, in my Treatise 'On the Undulatory Theory applied to the Dispersion of Light' (1841), I endeavoured to investigate the great problem of the explanation of the unequal refrangibility of light on the principles of the undulatory theory, as proposed by M. Cauchy about 1830, by numerical comparison with the indices observed, more especially in cases of the most highly dispersive media then examined.

The general result then arrived at was, that while the theory applied perfectly through an extensive range of media of low and moderate dispersive power, it did not apply well to those of higher ; and to the highest in the scale (which of course formed the true test of the theory) it did not apply within any allowable limits of accuracy. Since that time little has been done towards prosecuting the subject.

In the *experimental* part of the inquiry, about 1849, I had observed the indices for a few new media*; but these were not high in the scale ; yet though perhaps thus of little importance, I have now thought it as well to go through the calculation for them : the results are of the same general character as just described.

Soon after, finding that my friend, the Rev. T. P. Dale, F.R.A.S., was desirous to carry on some researches of this kind, I placed at his disposal the apparatus with which I had determined all my indices†.

In 1850 that gentleman communicated to the Royal Astronomical Society a short general account of his observations ‡ relative to some substances not very high in the scale.

* See British Association Reports, 1850, Sect. Proc. p. 14.
† Described and figured, British Association Reports, 1839.
‡ Notices, vol. xi. p. 47.

In 1858, Mr. Dale, in conjunction with Dr. J. H. Gladstone, F.R.S., presented to the Royal Society* a valuable series of determinations, evincing highly interesting results relative to the change of refractive power in various substances under different temperatures.

None of these media being high in the scale, they have little bearing on the main object of my inquiries. In two cases (viz. water and alcohol) the indices agree so closely with mine, that it was not worth while to recalculate them. In two other cases I have carried out the numerical comparison, which affords a good agreement with the theory.

Very recently the same gentlemen have, however, published some observations on several other media, especially phosphorus, a substance at the very summit of the scale, for which I had long been extremely desirous to obtain some determinations of indices†.

Among these results only two sets are in a form in which they can be made available for comparison with theory. These are the indices for the standard rays in bisulphide of carbon, and for solution of phosphorus in that medium, which I have now calculated theoretically.

The results (given in the sequel) in both cases indicate discrepancies between theory and observation too great to be due to any reasonable allowance for error ; and we are confirmed in the conclusion before arrived at, that, *for highly dispersive substances, the theory,* IN ITS PRESENT STATE, *is defective.*

But these comparisons are all made by means of the same formula employed in my former researches, viz. that derived from Cauchy's theory by Sir W. R. Hamilton, which he communicated to me, and which I explained in a paper in the Philosophical Magazine‡.

Considering the unsatisfactory condition in which the question was left when tried by the test of the higher media in my former inquiries, it is a matter of some surprise that in the long interval since the publication of those results no mathematician has been induced to *revise the theory.* Some criticisms indeed were advanced by Mr. Earnshaw§, and others by Prof. Mosotti and the Abbé Moigno‖, bearing on the general principle. Sir W. R. Hamilton's formula in particular was founded on certain assumptions confessedly but *approximate.* It remains then a promising field for inquiry to analysts, whether a better formula might not be deduced, or other improvements made in the general theory, by which a method applying so well to lower cases might be made equally successful for the higher.

Results of calculation, for Ether, Hydrate of Phenyle, Oils of Spikenard, Lavender and Sandal-wood, Benzole, Bisulphide of Carbon, and Solution of Phosphorus in that medium.

Three indices assumed from observation, viz. μ_B, μ_F, and μ_H, give the medium constants, viz.

$$D = \mu_F - \mu_B,$$
$$D' = \mu_B + \mu_H - 2\mu_F.$$

* Phil. Trans. 1858.   † See Phil. Mag. July 1859.
‡ Vol. viii. N. S. March 1836.   § See Phil. Mag. April 1842 and August 1842.
‖ See British Association Reports, 1849, Sect. Proc. p. 8.

The values of the wave-length constants A and B for each ray, independent of the medium, are taken from my Treatise (Undulatory Theory applied to Dispersion, &c., Art. 270). Combining these, we obtain AD and BD′ for each ray in the medium.

Thence Sir W. R. Hamilton's formula (*ib.* Art. 237) gives for any ray,

$$\mu = \mu_F \pm (AD + BD');$$

the upper sign being used for rays above F, the lower for those below.

Ether.—Dale and Gladstone.

Ray.	μ.		Difference.
	Observation.	Theory.	
B	1·3545		
C	1·3554	1·3544	—·0010
D	1·3566	1·3566	—·0000
E	1·3590	1·3586	—·0004
F	1·3606		
G	1·3646	1·3646	·0000
H	1·3683		

Hydrate of Phenyle.—Dale and Gladstone.

B	1·5416		
C	1·5433	1·5428	—·0005
D	1·5488	1·5495	+·0007
E	1·5564	1·5567	+·0003
F	1·5639		
G	1·5763	1·5772	+·0009
H	1·5886		

In both these media, of low dispersive and refractive power, the accordances of theory and observation are sufficiently close.

Oil of Lavender.—Powell.

B	1·4641		
C	1·4658	1·4632	—·0026
D	1·4660	1·4678	+·0018
E	1·4728	1·4726	—·0002
F	1·4760		
G	1·4837	1·4848	+·0011
H	1·4930 ?		

Oil of Sandal-wood.—Powell.

B	1·5034		
C	1·5058	1·4988	—·0070
D	1·5091	1·5062	—·0029
E	1·5117	1·5102	—·0015
F	1·5151		
G	1·5231	1·5271	+·0040
H	1·5398 ?		

Oil of Spikenard.—Powell.

Ray.	μ.		Difference.
	Observation.	Theory.	
B	1·4732		
C	1·4746	1·4744	−·0002
D	1·4783	1·4082	−·0001
E	1·4829	1·4826	−·0003
F	1·4868		
G	1·4944	1·4945	+·0001
H	1·5009		

Benzole.—Powell.			
B	1·4895		
C	1·4961	1·4907	−·0054
D	1·4978	1·4965	−·0013
E	1·5041	1·5029	−·0012
F	1·5093		
G	1·5206	1·5210	+·0004
H	1·5310		

In oil of lavender and of sandal-wood there was some indistinct-ness in the line H which renders its index a little uncertain. It may be owing to this circumstance that the assumption of that index may have occasioned the discrepancy between theory and observation.

In oil of spikenard the accordance is good. In benzole the discre-pancies are too great.

Bisulphide of Carbon.—Dale and Gladstone.

Ray.	μ.		Difference.
	Observation.	Theory.	
B	1·6177		
C	1·6209	1·6169	−·0040
D	1·6303	1·6251	−·0052
E	1·6434	1·6425	−·0009
F	1·6554		
G	1·6799	1·6807	+·0108
H	1·7035		

Phosphorus dissolved in Bisulphide of Carbon.—
Dale and Gladstone.

B	1·9314		
C	. . . . . .	1·9298	
D	1·9527	1·9522	−·0005
E	1·9744	1·9726	−·0018
F	1·9941		
G	2·0361	2·0363	+·0002
H	2·0746		

In the first of these media the differences are greater than can be fairly allowed to errors of observation.

In the second case it is yet more clearly apparent that the theory is defective. The ray C was not observed; but the theoretical index is evidently in error to a large amount, as it is even lower than that of B. The indices for D and C are perhaps within the limits of error; but that of E is too much in defect to be allowed.

GEOLOGICAL SOCIETY.

[Continued from p. 402.]

March 28, 1860.—L. Horner, Esq., President, in the Chair.

The following communications were read:—

1. "Notes about Spitzbergen in 1859." By James Lamont, Esq., F.G.S.

Mr. Lamont cruised about the southern coasts of Spitzbergen in his yacht during the summer and autumn of 1859. He first visited Edge's Land, which is composed of horizontal strata of limestone, shale, and sandstone, with some coal. One of the glaciers on this coast has a frontage of 30 miles. Deeva Bay was explored throughout. Black Point yielded some Carboniferous fossils. The Thousand Isles are composed of greenstone, sometimes columnar. Stour Fiord and Walter Thymen's Straits were next visited. The shores consist of the same kind of horizontal strata, with trap-rocks. Bell Sound and Ice Sound on the west coast, were also examined; the former has high hills of grey fossiliferous limestone all round it; the fossils, as determined by Mr. Salter, prove to be all Carboniferous. At various points on the coast and islands of southern Spitzbergen Mr. Lamont found bones of whales at elevations of 10 to 100 feet above the sea, and at distances of from a few yards to half a mile inland. The bones are sometimes imbedded in banks or moss. Drift-wood (pine) also abounds; some of it lies 30 feet above high-water-mark.

In the supplement to this paper, Mr. Horner supplied a description of the rock-specimens brought from northern Spitzbergen by Parry and Foster in 1827. From the evidence thus afforded it appears that the islands and mainland about the entrance of Waigatz Straits consist of granitic and gneissic rocks with quartz-rock and crystalline limestones, — possibly the altered equivalents of the Carboniferous sandstones and limestones of southern Spitzbergen. A list of the recent shells sent by Mr. Lamont from Spitzbergen was supplied by Mr. Woodward. Prof. Huxley gave the result of his examination of the bones—chiefly whale, white whale, and walrus. Mr. Prestwich described the gravels from Bell Sound— which consist chiefly of clay-slate, hornblende-slate, and mica-slate. Lastly Mr. Salter determined the following fossils—from the grey limestone of Bell Sound, *Athyris* or *Spirifer*, a large species, *Productus costatus, P. Humboldtii, P. mammatus* and another species of *Productus, Camarophoria, Spirifer Keilhavii, Streptorhynchus crenistria, Zaphrentis ovibos, Stenopora, Syringopora, Fenestella,* specimens of a new genus allied to the last, and some Sponges;— from the dark-coloured limestone of Black Point, Edge's Land,

Nucula, a small *Aviculo-pecten*, and *Spirifer*. A large *Aviculo-pecten* probably from the same locality also occurs; and one weathered block of white limestone, perhaps from Bell Sound, yields *Spirifer alatus*, a small *Productus* like the *P. horridus* figured by De Koninck as brought from Spitzbergen by M. Robert, and a large foliaceous *Stenopora*. These last, with a specimen of *Spirifer cristatus*, on another loose block, are the only forms having a Permian aspect in the collection made by Mr. Lamont.

2. " On the so-called Wealden Beds at Linksfield, and the Reptiliferous Sandstones of Elgin." By C. Moore, Esq., F.G.S.

When visiting the section at Linksfield, near Elgin, in the autumn of 1859, the author recognized a similarity of appearance between the shales and thin limestone-bands at Linksfield and those of the Bone-bed series (at the base of the Lias) at Pylle Hill, near Bristol, at Aust Passage and at Penarth, on the Severn, and at the Uphill cutting on the Great Western Railway. Giving in detail the sections at Pylle Hill and at Linksfield, the author pointed out some close lithological resemblances, and stated that he recognized the " white lias," the " Cotham marble," the " bone-bed," and the gypseous clay-bands of the south in the quarry at Linksfield. *Cyprides, Estheriæ,* remains of *Hybodus, Lepidotus, Acrodus,* and *Plesiosaurus, Mytilus, Modiola, Unio,* and *Cyclas,* from the Links-field beds, were among the palæontological evidences which the author brought forward as supporting his correlation of the beds in question.

He next offered some observations on the red layer of clay, sand, and stones intercalated between the Linksfield shales and the corn-stone, and, not accepting Capt. Brickenden's opinion of its having been thrust in by the action of ice against the escarpment during the formation of the boulder-clay, he suggested that an early glacial period, contemporaneous with the Lower Lias, destroyed some of the lower shales and limestone of Linksfield, leaving their remnants imbedded in a red drift to be covered by the succeeding undisturbed deposits of the bone-bed series.

Mr. C. Moore next remarked that the Cornstone at Linksfield, on which all the above-mentioned beds rest, might possibly be of Triassic date, as he had observed on the flanks of the Mendips and elsewhere a stone of a similar aspect, belonging to the Trias, and occasionally yielding remains of Reptiles and Fishes ; to this rock the author refers the druidical stones of Stanton Drew. Some observations on the discovery of reptilian and mammalian teeth in a Triassic deposit near Frome, by the author, on the possible relations of some of these to the *Reptilia* found in the Lossiemouth sandstone, and on the probable Secondary age of the latter, concluded the paper.

LXIV. *Intelligence and Miscellaneous Articles.*

NEW SECONDARY PILE OF GREAT POWER.   BY M. G. PLANTÉ.

JACOBI proposed recently the use of secondary electric currents for telegraphic purposes, and Planté had suggested the substitution of electrodes of lead for those of platinum in these batteries. A more

extended study has convinced him of their use. He states that a battery with electrodes of lead has $2\frac{1}{2}$ times the electromotive force of one with electrodes of platinized platinum, and six times as great as that of one with ordinary platinum. This great power arises from the powerful affinity which peroxide of lead has for hydrogen, a fact first noticed by De la Rive. The secondary battery which he recommends has the following construction. It consists of nine elements, presenting a total surface of ten square metres. Each element is formed of two large lead plates, rolled into a spiral and separated by coarse cloth, and immersed in water acidulated with one-tenth sulphuric acid. The kind of current used to excite this battery depends on the manner in which the secondary couples are arranged. If they are arranged so as to give three elements of triple surface, five small Bunsen's cells, the zincs of which are immersed to a depth of seven centimetres, are sufficient to give, after a few minutes' action, a spark of extraordinary intensity when the current is closed. The apparatus plays, in fact, just the part of a condenser; for by its means the work performed by the battery, after the lapse of a certain time, may be collected in an instant. An idea of the intensity of the charge will be obtained by remembering that to produce a similar effect it would be necessary to arrange 300 Bunsen's elements of the ordinary size (13 centimetres in height), so as to form four or five elements of $3\frac{1}{2}$ square metres of surface, or three elements of still greater surface. If the secondary battery be arranged for intensity, the principal battery should be formed of a number of elements sufficient to overcome the inverse electromotive force developed. For nine secondary elements about fifteen Bunsen's cells should be taken, which might, however, be very small.

From the malleability of the metal of which it is formed, this battery is readily constructed; by taking the plates of lead sufficiently thin, a large surface may be placed in a small space. The nine elements used by Planté are placed in a box 36 centimetres square, filled with liquid once for all, and placed in closed jars; they may also be kept charged in a physical cabinet, and ready to be used whenever it is desired to procure, by means of a weak battery, powerful discharges of dynamic electricity.—*Comptes Rendus,* March 26, 1860.

NOTE ON THE USE OF SULPHATE OF LEAD IN VOLTAIC COUPLES.

M. Edm. Becquerel describes a modification of the sulphate of lead battery invented by his father, M. Becquerel.

Sulphate of lead has the property, when it is made into a paste with a saturated solution of chloride of sodium, of becoming hard and compact; other chlorides exert a similar action. Cylinders may be moulded of this paste, if a rod of copper, lead, tin-plate, or even of gas-coke be placed in the centre. These cylinders, when dry, are permeable to a liquid conductor in which they are placed, and in contact with zinc form a constant current. Plates may also be formed of this substance; and when placed at the bottom of a vessel resting on a conducting support of copper, lead, or tin-plate, a piece of zinc being suspended above them, and the vessel filled with aci-

dulated water or solution of chloride of sodium, a constant couple
with a single liquid is formed, without a diaphragm.   But usually
the cylindrical form, combined with the use of a cloth or porcelain
diaphragm, is preferable.

All samples of sulphate of lead are not alike, probably from the
presence of foreign admixtures ; some become very hard, others do
not acquire a sufficient resistance.   Without knowing on what this
depends, a mixture of 100 grammes of dried and pounded sulphate
of lead, 20 to 30 grammes of chloride of sodium, and 50 cubic centi-
metres of saturated solution of chloride of sodium, gives good re-
sults : the addition of 20 to 25 grammes of oxide of lead (minium
or massicot) increases the hardness of the mass.   Another method
of using the different sulphates of lead is perhaps preferable.   It
consists in coating the freshly moulded sulphate of lead with a thin
layer of plaster.   This mass being placed in a liquid in the interior
of a hollow zinc cylinder, constitutes a couple ; in this way the sul-
phate does not get out of shape, nor is there any necessity for a
diaphragm ; the plaster serves this purpose, and prevents the con-
tact of the zinc and the reduced lead.

In these couples either water acidulated with sulphuric acid, or
solution of chloride of sodium may be used ; in the latter case the
electromotive force is somewhat greater than in the former, but the
solubility of sulphate of lead in it causes a deposition of reduced lead
on the zinc, which must be removed from time to time.   With
acidulated water this is not the case.

The electromotive force of these new couples, as compared with
that of a zinc-platinum couple, water containing one-tenth of acid,
and nitric acid, is as follows :—

Couple with nitric acid   100
Couple with sulphate of copper . . . .   58 to 59

| Couple with compact per-
meable sulphate of lead
and amalgamated zinc | With solution of chloride of
sodium . . . . | 28 to 30 |
| | With water acidulated with
sulphuric acid. . . . . | 27 |

In the first moments of the action the electromotive force de-
pends on the nature of the conductor in contact with the sulphate
of lead ; but as soon as there is metallic lead reduced, it attains a
constant value.   When these couples are in operation, the sulphate
of lead is reduced to the metallic state—the sulphuric acid from it
forming sulphate of zinc : the lead may be obtained by fusion.
From the chemical equivalents we readily get the relative weights
which the two electrodes must possess in order to have a constant
current; 100 grammes of zinc require 470 grammes of sulphate of lead.

These solid masses, permeable to liquids and employed as negative
electrodes, by preventing polarization play the same part as the per-
oxides of lead or manganese, nitric acid, and reducible metallic salts ;
but their resistance to conductivity, which varies with the progress
of the reduction, prevents the application of these couples with a
single liquid to the same uses as nitric acid couples.   They may,
however, be used with advantage where piles of great resistance
and long duration are required.— *Comptes Rendus,* April 2, 1860.

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END OF THE NINETEENTH VOLUME.

ALERE FLAMMAM.

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